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The Local Reaction to Unauthorized Mexican Migration to the US

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Abstract

We study the political impacts of unauthorized Mexican migration to the United States. Our identification strategy relies on two shift-share instruments that combine variation in migration inflows and migrant networks using data on more than 7 million likely unauthorized migrants who obtained consular IDs. We identify evidence of conservative electoral and policy responses at the level of a US county. Unauthorized migration significantly increases the vote share of the Republican Party in federal elections and decreases total public expenditure. We also find that the allocation of public expenditure shifts away from education towards policing and the administration of justice. We find evidence in favor of three interrelated mechanisms: economic grievance, reflected in formal job loss in "migrant-intensive" sectors and an associated increase in the number of poor people; out-migration, White flight, and population decline; and an increase in out-group bias, as manifested in reduced moral universalism. Unauthorized migration inflows have no discernible impact on total employment, average wages, unemployment, or crime rates. We find some evidence to suggest that the political and socioeconomic impacts of unauthorized migration are smaller in counties that have more progressive taxation or a more generous social safety net, suggesting that these policies can facilitate job switching and prevent a change in values.

Keywords: unauthorized migration, political economy, public expenditures, elections
JEL Codes: D72, F22, H7, H53, J61, J15

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‡Analyst, US Department of Defense. kara@rosscamarena.com. This is a work of independent research. The opinions expressed in this work are the authors' own and are not a reflection of the official position of the US Government or Department of Defense.

1 Introduction

How do citizens of wealthy countries respond politically to immigration from poorer countries? The weight of evidence points at a conservative, anti-immigrant response.[1] Studies from Europe find that inflows of refugees and migrants—especially those who are poorly educated and Non-European—tend to boost support for right-wing parties (Barone et al., 2016; Dinas et al., 2019; Edo et al., 2019; Halla et al., 2017; Mendez and Cutillas, 2014; Otto and Steinhardt, 2014). Similarly, studies from the United States document a conservative reaction to European migrants in the early 20th century (Tabellini, 2020) and to "low-skilled" migrants in more recent years (Mayda et al., 2022a,b). However, there is evidence of heterogeneous effects. In Europe, sustained and voluntary contact with refugees shifts political attitudes to the left (Dustmann et al., 2019; Steinmayr, 2021). In the US, the presence of "high-skilled" migrants does not generate a conservative response (Mayda et al., 2022a,b). Thus, the political reaction to immigration seems to be shaped by the characteristics of migrants and the type of interaction they experience with local residents.

Whether an inflow of unauthorized migrants provokes a political response similar to an influx of refugees or poorly educated migrants is still unknown. To shed light on this question, we study the reaction of US citizens to inflows of unauthorized Mexican migrants between 2010 and 2020. We focus on the Republican party's vote share in federal elections and public spending at the local level. Unauthorized Mexican migrants constitute an especially salient group in the US political landscape. At 11 million people, the Mexican-born population in the US constitutes the largest diaspora in a single country in the world (United Nations Department of Economic and Social Affairs, 2021). Roughly 40% of them do not have regular migration status (Passel and Cohn, 2018; Gonzalez-Barrera, 2021). These migrants are invoked widely in US political debate, but there is little formal evidence about how they affect national politics and policies—probably because it is challenging to observe unauthorized, relative to authorized, migrants. By gaining access to unique consular data on Mexican migrants and combining it with rich US administrative data, we document these political effects and make progress on uncovering theoretical mechanisms.

We identify the reaction by predicting plausibly exogenous inflows of migrants using a confidential dataset on over 14 million consular identification cards issued to 7.4 likely unauthorized Mexican citizens living in the US between 2002 and 2020. Given that there is no systematic data on Mexicans who migrate without regular status, related literatures have relied on indirect sources, like the American Community Survey (ACS)(Borjas and Cassidy, 2019)—which proxies unauthorized migrants with low education attainment—or

[1] See Alesina and Tabellini (2021) for a comprehensive literature review.

apprehensions at the border (Hanson and Spilimbergo, 1999). However, these samples are not likely to be representative, as not all unauthorized migrants are poorly educated, and those who are captured at the border may differ from those who enter successfully due to systemic factors. The consular data proxies the population of interest better. Mexican nationals with regular status can obtain identification from the US government, so only unauthorized Mexicans need Consular IDs. Our assumption is that consular cardholders are predominantly unauthorized, as other scholars with similar data have assumed (Allen et al., 2018; Caballero et al., 2018; Bhandari et al., 2021; Dinarte Diaz et al., 2022; Albert and Monras, 2022).[2]

Another advantage of the consular data is that it helps us address selection bias. Migration decisions are not random. Migrants may choose to settle in places that are politically welcoming and have economic opportunities, making it difficult to isolate the effect of interest. The geographic granularity and coverage of the consular data allow us to circumvent such selection bias. Based on the Mexican municipality of origin and US county of residence of cardholders, we construct pre-existing migrant networks that are the basis of two different shift-share strategies. Our preferred shift-share specification uses a leave-one-out approach. That is, we interact the initial municipality-county shares with migration inflows from every Mexican municipality to the US, net of those migrants who actually established residence in the core-based statistical area (CBSA) of each county. Our second strategy is more demanding, exploiting push factors from Mexico. We use the same initial municipality-county shares, but we predict migration flows based on time-varying Mexican municipality characteristics. Both shift-share strategies identify the effect of migrants who go to counties in the US because of their migrant network. The compliers in our setting are people who settle in counties where they have strong networks, not necessarily where economic conditions are promising or where the marginal product of labor is highest. We do not study the effect of random migration flows.

The identifying assumption is that the predicted number of migrants impacts the outcomes of interest only by its effect on observed migration. Given that we exploit within county and state-period variation, we assume that the US county-level characteristics that attracted Mexicans from particular municipalities in the pre-period do not affect the evolution of economic, political, and social characteristics of the county in later periods. We claim that the shifters used in both strategies—leaving a CBSA out and predicted migration from time-varying municipality characteristics—are exogenous to the trajectory of the outcomes of interest (Goldsmith-Pinkham et al., 2020; Borusyak et al., 2022). We present evidence

[2] In Section 2.2 we show evidence indicating that our sample is representative of the unauthorized Mexican population in the US.

against pre-trends, differential trends, and nonrandom exposure to migration (Borusyak and Hull, 2020). Finally, the consistency among the estimates obtained with the two instruments supports the plausibility of our identifying assumption.

Our main results point to a conservative response in voting and policy. Recent inflows of unauthorized migrants increase the vote share for the Republican Party in federal elections, reduce local public spending, and shift it away from education towards law-and-order. A mean inflow of migrants (0.4 percent of the county population) boosts the Republican party vote share in midterm House elections by 3.9 percentage points. Our results are larger but qualitatively similar to other scholars' findings of political reactions to migration inflows in other settings (Dinas et al., 2019; Dustmann et al., 2019; Harmon, 2018; Mayda et al., 2022a). The impacts on public spending are consistent with the Republican agenda. A smaller government and a focus on law-and-order are two of the key tenets of conservatism in the US. A mean inflow of migrants reduces total direct spending (per capita) by 2% and education spending (per child), the largest budget item at the local level, by 3%. The same flow increases relative spending on police and on the administration of justice by 0.23 and 0.15 percentage points, respectively. These impacts on relative spending suggest that the decrease in total expenditure does not simply reflect a reduction in tax revenues but also a conservative change in spending priorities. These results are in line with theories suggesting that lower spending in response to immigration can be due to coordination failures, heterogeneity of preferences and out-group bias (Alesina et al., 1999; Hanson et al., 2007; Facchini and Mayda, 2009; Card et al., 2012; Hainmueller and Hopkins, 2014; Alesina et al., 2022; Derenoncourt, 2022).

To uncover the underlying mechanisms, we also examine outcomes relating to economic activity, residential sorting, and moral attitudes. In terms of economic activity, we find that inflows of recent migrants reduce formal employment in construction and hospitality and leisure—"migrant-intensive" sectors—increase the number of people in poverty, and marginally reduce GDP per capita and median household income. In terms of residential sorting, they increase out-migration and reduce total and White population. In terms of moral values, they cause a decline in relative universalism, indicating that bias against "out-group" members increases.

Taken together, these results provide evidence in favor of both cultural and economic reasons behind the backlash to immigration. While we cannot detail the timing behind the response, our interpretation is that there are three different yet related reactions. (1) Unauthorized migrants may displace formal workers in industries with large informal sectors that are easily accessible to those without work papers, like construction and hospitality. Most of these displaced workers eventually find employment in other sectors that are less "migrant-intensive" or less informal, like manufacturing. This job switching explains the lack of effects on unemployment and total formal employment. However, the temporary formal job loss probably prompts the transitory increase in poverty. (2) Citizens or established residents develop less favorable opinions towards migrants. The fact that the response to out-groups is more intense in counties that experience more poverty or formal job loss suggests that economic grievance is partially driving it, perhaps in concert with political entrepreneurship (Alsan et al., 2020; Baccini and Weymouth, 2021; Hopkins et al., 2023). (3) Similar to the impact of the Great Migration (Boustan, 2010; Derenoncourt, 2022; Shertzer and Walsh, 2019; Tabellini, 2018), a subset of the population, particularly White residents, may simply not want to interact with unauthorized migrants and thus choose to leave their places of residence. We observe out-migration even in counties that do not experience a sharp increase in poverty.

Finally, we suggest policy implications. Many of the documented effects of unauthorized migration are muted in counties with a stronger social safety net, proxied by more progressive taxation (ratio of income to sales tax) or more redistribution relative to poverty (ratio of TANF to poverty).[3] For example, in counties with more progressive taxation, the effect of unauthorized migration on the vote share of the Republican Party is halved, there is job gain and wage increase in construction, and there is an increase in universalism. Our interpretation is that these counties are better able to compensate those who lose economically. Progressive taxation does not mute the effect on out-migration, suggesting that residential sorting occurs irrespective of economic factors.

This paper contributes to different strands of literature. It is, to the best of our knowledge, the first to analyze the political and social impacts of flows of Mexican migrants, whether authorized or not, to the US. There is extensive literature analyzing the labor market impacts of inflows. Scholars have found that inflows of Mexican migrants have either no overall economic impact or small negative impacts in certain sectors and regions (Hanson, 2009; Monras, 2020; Clemens et al., 2018; Blau and Mackie, 2017). Wage differentials between the US and Mexico remain a compelling explanation for the migration flows (Hanson and Spilimbergo, 1999).

This paper also expands the literature on the political impacts of contemporary immigration to the US by documenting the specific effect of unauthorized Mexican migrants—the largest group of unauthorized migrants (Ward and Batalova, 2023). Two recent articles explore similar topics. Mayda et al. (2022a) study the impact of "high-skilled" and "low-skilled" immigration on political outcomes and find that the former shifts voters to the Democrats,

[3] Temporary Assistance for Needy Families (TANF) is a federal cash transfer program. States have discretion over the size of the transfer.

while the latter shifts voters to the Republicans. Mayda et al. (2022b) extend the analyses and finds that local expenditures decrease with "low-skilled" immigration and increase with "high-skilled" immigration.[4] We build on these two papers by specifically studying unauthorized Mexican migrants. This group is sizable, a focus of political contention, and theoretically distinct from regular migrants, refugees, or poorly schooled migrants. One difference is that unauthorized migrants experience additional barriers to formal employment. The compensation package that employers offer them is likely to be lower than that of authorized migrants. Hence, in sectors with high levels of informality there is greater scope for labor substitution/displacement relative to other migrant groups typically studied in literature.

Our paper also sheds light on the mechanisms explaining the shift to the right due to immigration influxes. We highlight the distinctive role of economic and cultural/ideological factors and the link between them and demographic change. We identify three prominent possible explanations informed by recent literature reviews (Alesina and Tabellini, 2021; Hanson, 2009; Rodrik, 2021). The first is that, despite aggregate gains in the long run, migration causes labor market frictions. Migrants compete with natives with similar skills, resulting in higher unemployment or lower wages in the short run for these groups (Borjas et al., 2012; Burstein et al., 2020; Cortes, 2008; Hanson, 2009). Politicians, in turn, may play to the worse-off group of voters by promoting policies against migrants (Alsan et al., 2020; Baccini and Weymouth, 2021; Hopkins et al., 2023; Müller and Schwarz, 2023).

The second explanation relates to heterogeneity. The hypothesis is that migrants' otherness prompts exclusionary attitudes (Brader et al., 2008), potentially offsetting more welcoming attitudes arising from increased contact (Enos, 2014). For example, established residents may prefer lower redistribution to ethnically different people (Alesina et al., 1999; Alesina and Giuliano, 2009) or they may want to preserve their power in a polarized environment (Bazzi et al., 2019). Established residents who are unwilling to interact with newcomers, or prefer to preserve the composition of their communities (Card et al., 2012), might decide to move, causing demographic changes. Boustan (2010) and Shertzer and Walsh (2019) document the movement of White people from northern US states out of their communities of residence as a response to the Black Great Migration. Since those who left were comparatively wealthier, "White flight" caused a decline in revenues and public expenditures (Tabellini, 2018).

The third explanation has to do with attitudes and (mis)perceptions. Estabished res-

[4]Two other papers uncover related associations. Baerg et al. (2018) observe that, in the state of Georgia, counties with higher fractions of unauthorized migrants tend to vote more Republican. In contrast, Hill et al. (2019) note that changes in the fraction of Hispanics between 2012 and 2016 negatively correlate with changes in the Republican vote share in precincts of seven states.

idents may assign negative characteristics to migrants (Hainmueller and Hopkins, 2014; Alesina et al., 2022; Facchini and Mayda, 2009). Negative perceptions may include the idea that migrants threaten residents' jobs (Ajzenman et al., 2022), increase crime (Ajzenman et al., 2021), or do not contribute economically to their place of residence. These attitudes lead citizens to vote for anti-migrant politicians and policies. Rozo and Vargas (2021) demonstrate how politicians can use these (mis)perceptions of migrants strategically to gain office, and Abrajano and Hajnal (2017), Couttenier et al. (2021), and Djourelova (2023) explore how the media can enhance these negative perceptions of immigrants. The logic of why established residents acquire these negative associations is similar to a class of explanations for immigrant backlash in political and social psychology driven by inter-group threat (Riek et al., 2006; Mutz, 2018). Of particular importance is the finding in this literature that economic vulnerability further enhances the perception of threat (Dustmann et al., 2019; Margalit, 2019).

This paper helps to unpack the relative importance of these theories. We find evidence in support of the idea that economic factors are only partially responsible for the conservative political reaction and that preferences, unrelated to economic factors, are relevant. The economic impacts we estimate are small, concentrated in a few sectors, and do not translate into an average decline in employment, suggesting that there is job switching, and there are economic winners. We document, moreover, that values and economic grievances are probably related, since the decline in relative universalism is steeper in counties with higher changes in poverty. Economic factors do not seem to explain out-migration. However, the fact that relatively wealthier natives are more likely to move as a result of inflows of migrants brings down median income. The null results on crime weaken an explanation based on actual threat. Nevertheless, due to a lack of data, we cannot rule out that perceived threat motivates the conservative response. We present preliminary evidence to counter the backlash against immigration. Our findings imply that a more robust safety net can protect native workers affected by migration, mitigate poverty, and reduce out-group bias, curtailing the support for reactive politics and policies.

In the remainder of the paper, we introduce a novel dataset and demonstrate its appropriateness for our question of interest (Section 2). We explain our shift-share instruments and examine the key identifying assumption (Section 3). Then we establish that flows of migrants shift voters toward the Republican party and drive more conservative public spending (Section 4). We establish the robustness of our results (Section 5). In Section 6, we explore three sets of mechanisms and highlight economic disruptions, demographic change, and a shift in values. Section 7 explores the heterogeneous effects across measures of the social safety net and discusses implications.

2 Data

Since the mid-1800s, the Mexican government has offered identification cards to its citizens living in the United States, regardless of their immigration status (Laglagaron, 2010; Márquez Lartigue, 2021). With the Patriot Act in 2001, requirements for identification became more stringent in the United States, so migrants without immigration authorization had even more limited access to US-issued identification cards, making them virtually unable to access some basic services, such as banking or housing (Bruno and Storrs, 2005; Mathema, 2015). In 2002, the Mexican Consular Services responded by straightening the requirements to obtain an ID. Before then, the identification was a piece of paper. The new (current) consular card, called "Matrícula Consular de Alta Seguridad," is a formal plastic card with several authentication mechanisms (Bruno and Storrs, 2005; Massey et al., 2010). Every Mexican person, regardless of age, is eligible to get an ID. To obtain one, a person must show proof of residence and nationality[5] and pay a fee of 35 USD. IDs are valid for five years, and the renewal process is identical. There are no immigration status requirements. While there is no data, anecdotal evidence suggests that nearly everyone with the necessary Mexican documentation is able to get the ID. Issuing consular IDs is central to consular activities, so much so that most of the personnel working in the consular network are employed issuing either passports or consular IDs.

The updated administrative database is the source of our data. The dataset has information on the municipality and date of birth, marital status, educational attainment, sector of employment, and US county and state of residence of cardholders. The National Institute of Mexicans Abroad (IME) intermittently publishes aggregated versions of this data.[6] However, the aggregated dataset does not show specific people, nor does it allow construction of Mexican municipality-US county pairs. The Mexican Ministry of Foreign Affairs (SRE) has shared with us a confidential, detailed version of the dataset. It contains anonymized demographic information of every Mexican national who got an ID between 2002 and 2020. The SRE created an identification number that allows us to track people over time. This number has no relevant meaning nor is it linked in any form to other demographic information. The data consists of 16.7 million observations corresponding to 8.8 million individuals.

[5] To facilitate getting IDs, Mexican state governments have established offices near many consulates to help people retrieve birth certificates. Letters from churches can serve as proof of residence.

[6] At the time of writing this paper, the official site had inconsistent links to download the data. Here is the link for the 2018 information https://www.gob.mx/ime/acciones-y-programas/estadisticas-de-matriculas-de-personas-mexicanas-en-estados-unidos-2018

2.1 Constructing a Measure of Migration Flows with the Consular Data

There are two main challenges to estimating the number of newcomers from our data. It is necessary to (1) differentiate between renewals and first-timers and (2) make assumptions about the likelihood that an average newcomer applies for an ID and remains in the same county. We have taken the following approach. First, we count the number of new cards per person in 4-year intervals, 2007–10, 2011–14, and 2015–18.[7] This frequency is convenient because it allows us to observe the total (4-year aggregate) inflows of migrants during an election year. Moreover, there is evidence that cardholders tend to be newly arrived migrants, that the majority of newly arrived Mexican migrants obtain a card over a five-year period, and that cardholders tend to remain at least in the same state over those five years (Allen et al., 2018; Caballero et al., 2018; Massey et al., 2015). Second, for each one of these periods, we classify as newcomers the people who got an ID for the first time in a new core-based statistical area (CBSA), a geographical unit that encompasses several counties. The strategy, as opposed to counting solely the people who got an ID for the first time, considers migrants moving from one CBSA to another as newcomers. Third, we count only the observations with complete and consistent information regarding place of birth and county of residence. We estimate that 2.13 million newcomers arrived between 2007 and 2010, 1.3 between 2011 and 2014, and 0.95 between 2015 and 2018. As Appendix B shows, these figures are consistent with other common estimates (Passel and Cohn, 2018; Wassink and Massey, 2022). While the number of new unauthorized Mexican migrants has declined in recent years, large flows still persist.

To calculate the fraction that the migrants represent in every county, we divide the total number by the county population in the final year of the period—e.g. 2010, 2014, and 2018—using population estimates from the US Census. Figure 1 shows the national distribution of the fraction of unauthorized Mexican migrants for each period. The average is 0.69% for the first period, 0.4% for the second, and 0.28% for the third. During the time of our study, there were at least 10 migrants in 2,674 US counties,[8] around 88% of all US counties.

[7] We exclude the years 2002–2006, as we use them to create the shares for our instrument.

[8] To protect people living in areas with a very low number of migrants, we only consider counties with more than 10 migrants from 2002 to 2020. We also do not consider Alaska because the number and names of counties have changed significantly during our period of study.

Recent unauthorized Mexican migrants as share of county population

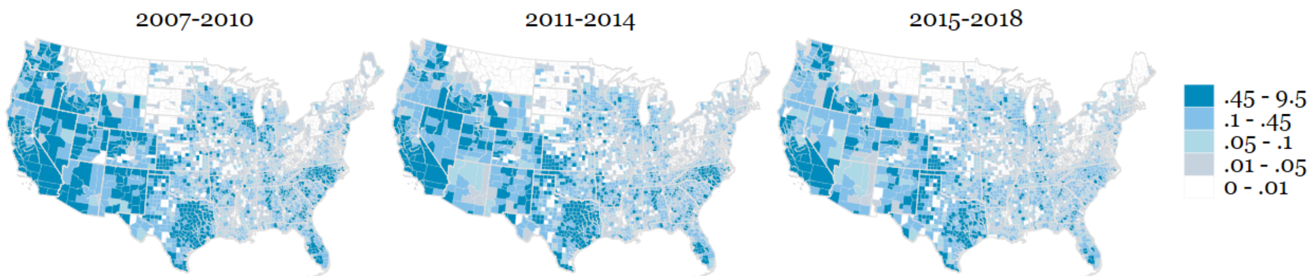

Figure 1: Map of observed number of newcomers. Sources: SRE and US Census Bureau, Population Division

2.2 Validity of Immigration Flows Constructed with the Consular Data

There is evidence that the consular dataset captures unauthorized migrants well. Because migrants with valid visas or work authorization have access to identification from US authorities, the working assumption among scholars using this data is that it captures predominantly unauthorized migrants (Bhandari et al., 2021; Caballero et al., 2018; Massey et al., 2010; Monras, 2015). Caballero et al. (2018) report a strong correlation between the log number of cards issued in each state between 2006 and 2010 with the log estimated number of Mexican-born residents obtained from the 2010 and 2011 American Community Surveys (ACS). We carry out a similar analysis using ACS 5 2006–10, 2010–14 and 2014–18 (Ruggles et al., 2022). Following the Allen et al. (2018), we consider a likely unauthorized newcomer Mexican migrant in the ACS 5 as those who were born in Mexico, with no US citizenship, with no college education, and who have been in the US for less than 4 years. Figure 2 plots the log of likely unauthorized migrants from our data and two repositories of ACS 5. The left panel uses data for the 2,674 counties covered by ACS 5, in the Social Explorer, and our data. [9] The right panel uses data for the 441 counties covered by ACS 5, in IPUMS (Ruggles et al., 2022), and our data. Our correlation coefficients are 0.85 and 0.82, respectively. The association is considerably weaker in areas with few migrants, probably due to low precision from the Social Explorer data. Further, Appendix C compares key demographic variables of 441 counties covered by ACS 5 and the consular data and finds no significant differences.

Another potential concern is that, even if the consular data measures unauthorized migrants, it also captures authorized migrants. If that were the case, it would be hard to

[9] Our estimate of recent Mexican migrants is the number of people born in Mexico multiplied by the county average share of migrants (from all countries) that arrived before 2000, for 2010, or before 2010, for 2014 and 2018.

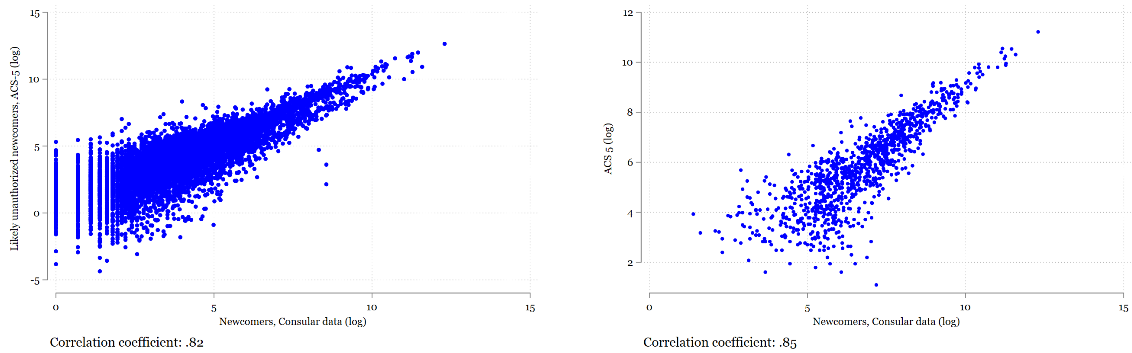

Figure 2: Correlation between ACS 5 and Consular Data

determine whether the effects we observe are due to authorized or unauthorized migration. In Appendix D, we explore this possibility. Using the detailed data from the 441 counties covered by from ACS 5 (IPUMS), we regress the estimate of unauthorized Mexican migration described before and an analogous estimate of authorized migration—constructed again following Allen et al. (2018)—on our preferred instrument. The correlation between our instrument and the estimate of unauthorized migrants is strong, whereas the relationship between the instrument and the estimate of authorized migrants is weak and barely significant.[10]

Selection bias at the county level is another threat to the validity of this data as a metric of unauthorized migration flows across counties and years. The potential problem is that unauthorized migrants in counties that change their policy environments could have a stronger (or weaker) incentive to request an ID. Our assumption is that migrants get consular IDs to access basic services, like banking or housing, and to send remittances to Mexico, almost regardless of the policy environment in their county of residence. In Appendix E we test this assumption by observing the evolution of IDs after some states made driver's licenses and non-driver IDs available to unauthorized migrants. While demand seems responsive to these changes, the effect seems transitory. In Appendix F, we follow East et al. (2022) and Alsan and Yang (2019) and carry out an analogous exercise after the activation date of Secure Communities, a program where local police submit individuals to federal authorities for deportation review. We fail to observe significant changes. Collectively, these two results suggest that demand for consular IDs is rather inelastic to the local policy environment in the medium term. Getting a consular ID is not only important to carry out regular tasks

[10]As Table 11 indicates, a marginal increase in the LOO instrument is associated with an expected increase of 0.553 in the proxy of unauthorized migrants—F statistic 192. In contrast, a marginal in the LOO instrument is associated with an expected increase of only 0.028 in the proxy of authorized migrants — F statistic 4.

in everyday life, but also a common, almost habitual, task that migrants do. In any case, the contemporary policy environment of a state or a county does not affect our instrument, as we rely on past networks and national inflows of migrants. Moreover, our specifications control flexibly for state-by-period fixed effects.

2.3 Dependent Variables

We use two sets of dependent variables in our primary analysis. First, we examine the impact of migrants on the vote share for the Republican Party in Congressional and presidential elections. The electoral data comes from Dave Leip's Atlas of US Presidential Elections. It lists the number and share of votes obtained by each of the two major US parties in every county for all federal elections: House, Senate, and presidential. We are interested in studying the effect of migration during midterms and presidential years. For midterms, we analyze the elections of 2010, 2014, and 2018. For presidential year elections, we analyze 2012, 2016, and 2020. Since the Senate renews only partially every election cycle, we focus on House and presidential elections. In our period of analysis, in terms of the popular vote, the GOP won two midterm elections (2010 and 2012) and one House election in a presidential year (2016).

Second, we examine public good provision with county-level revenue and expenditures, and focus on spending in public education, policing, and the judiciary. The data comes from the Annual Survey of State and Local Government Finances. Our goal is to investigate changes in policy at the local level, regardless of the specific government agencies that carry them out. Therefore, we aggregate all local expenditures, by topic, within each county. This includes spending by the county government, cities, townships, special districts, and independent school districts. Expenditure codes are consistent across these five types of agencies and facilitate a simple summation across government entities.

Since *a priori* it is ambiguous whether migration affects absolute or relative expenditure, we explore both. The absolute expenditures are the log of total expenditures per capita, in 2010 thousand dollars. For this and all other per capita measures, we use US Census data for county population. The relative expenditures for education, police, and justice are the shares they represent of total direct expenditures. By far, the largest expenditure item at the local level is education. On average, in our sample, 40% of the total direct expenditures within counties are for education. Other "productive goods and services" like sewage and highways, represent 3% and 4% respectively. One shortcoming of this dataset is that, except for school districts, it surveys all the counties only in years that end in 2 and 7. For the rest of the years, the estimates are based on a sample of around 15% of the total number

Table 1: Summary statistics

	Mean	Std	Min	Max	Obs	Counties	Data relative to end of periods
Newcomers, population fraction	.462	.591	0	9.404	8022	2674	0
Instrument, leave county out	.421	.572	0	3.822	8022	2674	0
Instrument, leave CBSA out	.404	.551	0	3.822	8022	2674	0
Instrument, push factors	.44	.595	0	3.651	8022	2674	0
Population, '000	116.5	348.1	.4	10061.5	8022	2674	0
Vote share GOP House, midterm	48.2	19.4	0	100	7995	2673	0
Vote share GOP House, Pres	47.2	19.9	0	100	8015	2673	2
Vote share GOP President	45.97	16.59	4.09	95.43	8022	2673	2
Total revenue, pc log	1.57	.39	-.02	4.18	5340	2634	2.5
Total (dir exp), pc log	1.54	.39	-.07	4.17	5340	2670	2.5
Edu (dir exp), pc 0-19 log	1.97	.34	.47	4.69	5330	2665	2.5
Edu (dir exp), share	40.69	11.89	0	89.47	5340	2670	2.5
Police (dir exp), pc log	-1.43	.5	-6.87	1.54	5338	2670	2.5
Police (dir exp), share	5.45	1.855	0	71.746	5340	2670	2.5
Judicial (dir exp), pc log	-2.95	.88	-10.12	-.31	5296	2662	2.5
Judicial (dir exp), share	1.401	.851	0	12.486	5340	2670	2.5

Column 1 is the mean of the variable. Column 2 is the standard deviation. Column 3 is the minimum. Column 4 is the maximum. Column 5 is the total number of country-period observations. Column 6 is the number of unique counties. Column 7 reflects the year for which we have data relative to our periods: 0 indicates that the data is for the years 2010, 2014, and 2018; 1 indicates that it is for the years 2011, 2015 and 2019; 1.5 (0.5) indicates it is a 1.5 (0.5) year average after the end of our periods. All the estimates (except county population) are weighted by county population. Sources: SRE, Dave Leip's US Election Data; Annual Survey of State and Local Government Finances; US Census.

of local agencies, all of which are in counties of more than a designated population—county governments of more than 100,000 people in 2010, for instance (Annual Survey of State and Local Government Finances, 2010). We use data for 2012 and 2017 and estimate the effect of newcomers in the period 2007–2010 on expenditures in 2012 and of newcomers in the period 2011–14 on expenditures in 2017. Table 1 presents the main statistics for these variables, the instruments, and the endogenous variable.

3 Empirical strategy

To estimate the effects of unauthorized migration on political outcomes, we require a source of exogenous variation. A simple comparison between counties with more and fewer unauthorized migrants would provide biased estimates, since the number of migrants that counties receive is potentially endogenous. For example, migrants may select into places that are more economically promising or more friendly toward migrants, and these factors in turn may vary with our outcomes of interest. To address this bias, we use two shift-share strategies that differ with respect to the measurement of the shifters.

Shift-share strategies are widely used in the fields of migration and labor. In short, they predict treatment by combining a measure of initial exposure that varies cross-sectionally, the *shares*, with an aggregated shock that varies in time, the *shift*. In our setting, initial county exposure is given by the networks of unauthorized migrants coming from different Mexican municipalities. Counties with stronger networks are those where a larger share of the migrants come from Mexican municipality M.

Our two shift-share strategies rely on the same shares but use different shifters. The main strategy uses, in the spirit of Tabellini (2020), a leave-one-out approach. Namely, the shifts are the inflows of all migrants arriving in the US from Mexican municipality M, excluding the flows to the county of interest. The second strategy, in contrast, predicts migration flows from every Mexican municipality M using time-varying push factors.

Equation 1 details the second stage estimation, common to both strategies:

$$Y_{cst} = \beta_0 + \beta_1 Recent\widehat{Mex}Migrants_{cst} + \psi_c + \eta_{st} + \epsilon_{cst} \quad (1)$$

where Y_{cst} are the outcomes of interest for county c in US state s during the 4-year period t. β_1 is the effect of the predicted unauthorized Mexican migrants as a share of predicted population. ψ_c are county fixed effects and η_{st} are state-period fixed effects.

Equation 2 is the first stage of this estimating equation, also common to both strategies:

$$RecentMexMigrants_{cst} = \gamma_0 + \gamma_1 Z_{cst} + \phi_c + \pi_{st} + u_{cst} \quad (2)$$

where Z_{cst} is the shift-share instrument, with either leave-one-out or push factor shifters, and ϕ_c are county fixed effects and π_{st} are state-period fixed effects.

The first step for both strategies is to construct the endogenous variable, the observed number of migrants, the way we described in Section 2.1. We count the unique new consular IDs in every US county during each of the three 4-year periods 2007–10, 2011–14, and 2015–18.[11]

The second step, again common for both strategies, is to create pre-period shares using the first five years of data (2002–2006). We count all the individuals who got a consular ID in every county C in this five-year period—following the same rejection rule regarding the CBSA duplication. We decompose this total number of migrants by county according to their municipality of origin M in Mexico. Migrants from our sample come from 2,449 municipalities, over 99% of the total. Then, we add up the migrants from each municipality living in all US counties during that period. Finally, we calculate the share of those migrants from municipality M that lived in each US county C. Thus, our initial shares are the proportion of migrants from municipality M who live in county C. For example, we counted 585 people from Alvarado, Veracruz, in the US from 2002–2006. Among them, 9.2% lived in Los Angeles County, CA, 7.5% in Ventura County, CA, and 5.8% in Milwaukee County, WI.

For the leave-one-out strategy, the next step is to multiply the original fraction of migrants from municipality M living in county C by the total number of migrants from municipality M who entered the US during that period, net of those that eventually settled in that county's CBSA. This is the leave-one-out component. There are a few counties that do not belong to any CBSA. For those, we leave out only the county itself. The product of the initial share and the new flow, leaving out the CBSA, is our leave-one-CBSA-out shift-share. For example, we count 550 people moving from Alvarado to the US between 2007 and 2010; 52 settled in Los Angeles' CBSA, 21 in Ventura's, and 93 in Milwaukee's. Thus, the predicted migration in each county is 46 ($0.092 \times (550–52)$), 39.8 ($0.075 \times (550–21)$), and 26.6 ($0.058 \times (550–93)$) respectively.

Last, we scale the leave-one-CBSA-out shift-share by the predicted population of the county. We use predicted population since the presence of unauthorized migrants could affect the population of the county. We follow Tabellini (2020) and calculate the predicted population by multiplying the population of the county in 2006 times the population growth

[11] To ensure uniqueness we drop likely change of address IDs in the same period. That is, when individuals get a new ID in the same period and a different county of the same CBSA, we cannot rule out a simple change of address. Therefore, we drop these records.

of similar counties in terms of the urban-rural classification in other regions of the US. Formally, the leave-one-out instrument is given by Equation 3.

$$Z_{cst} = \frac{1}{\widehat{P}_{cst}} \sum_{m} Sh_{mcs,2006} * O_{mt}^{-cbsa} \tag{3}$$

where $\widehat{P}_{cst}$ is predicted population, Sh fraction of migrants from Mexican municipality m in US county c in US state s during the pre-period 2002–2006. O_{mt}^{-cbsa} is the total migrants from municipality m in period t that migrated to the US, net of those who migrated to county's CBSA.

For the push factor strategy, we follow Munshi (2003) and predict the observed number of migrants from Mexican municipality M during each four-year period t using four types of time-varying variables: historical and contemporaneous climate and precipitation from University of Delaware (NOAA PSL and University of Delaware, 2022), infant, child, and maternal deaths and death rates from the Mexican Statistical Agency (INEGI) (Instituto Nacional de Estadística y Geografía, 2023d), poverty and several other social development indicators from the National Council for the Evaluation of Social Development Policy (CONEVAL) (Consejo Nacional de la Evaluación de la Política de Desarrollo Social (CONEVAL), 2023b,a), and indicators of economic activity, like the number of firms or total production, from INEGI's Economic Census (Instituto Nacional de Estadística y Geografía, 2023b,c,a). To avoid over-fitting, we select the most relevant predictors using LASSO. Since the number of migrants is censored at zero, we estimate a Poisson regression. Appendix I describes the variables used for this instrument in detail. Equation 4 describes this "zero stage" exercise

$$PredictedMigrants_{mt} = \alpha_0 + \mathbf{X}_{mt} + \xi_{mt} \tag{4}$$

where X_{mt} is the battery of municipality time-varying variables.

The instrument, thus, is given by interacting the predicted number of migrants from M in period t with the original pre-period shares. Equation 5 describes the instrument formally

$$Z_{cst} = \frac{1}{\widehat{P}_{cst}} \sum_{m} Sh_{mcs,2006} * Predicted\widehat{Migrants}_{mt} \tag{5}$$

where all the terms are just as in equation 3. Following the example above, we predict 781.3 people from Alvarado in the US between 2007 and 2010. The predicted migration in each of the main destination counties is LA 72.1 (0.092×781.3), Ventura 58.7 (0.075×781.3), and Milwaukee 45.4 (0.058×781.3).

In line with recent developments in two-stage least squares (2SLS) literature (Blandhol et al., 2022), we do not control parametrically for covariates. We only include county and state-by-period fixed effects.

3.1 Identifying Assumptions

To provide causal estimates, at least one of the components of shift-share designs must be exogenous (Borusyak et al., 2022). Given that we have panel data and exploit only within county variation, the exogeneity in this setting relates to changes in the outcomes, rather than to their levels. Our assumption is that the shifters in both strategies are exogenous. We argue that by excluding the CBSA of the county of interest or by using Mexican municipality push factors, the constructed shock is uncorrelated with any unobserved factors in the residuals.[12] For the leave-one-out instrument, a key component of this assumption is that the numbers of migrants are not spatially correlated among CBSAs (Borusyak and Hull, 2020).[13]

The main identifying assumption of shift-share designs with panel data is analogous to the parallel trends assumption of difference-in-differences estimators (Goldsmith-Pinkham et al., 2020; Cunningham, 2021). We assume that the observed effects in the variables of interest are solely due to the instrument via the endogenous variable. The argument is that, conditional on county and state-by-period fixed effects, predicted migration affects the evolution of electoral and policy outcomes only through observed migration.

There are two main threats to identification. (1) Our results would be biased if counties that received more Mexican newcomers were already on a different political and socioeconomic trend from those that received fewer Mexican newcomers. This would occur if either the variables of interest or other key regressors were on different trajectories or if the initial shares had persistent effects. This would be a violation of the parallel trends-like assumption.

(2) Our results would be biased if counties were non-randomly exposed to migration shocks. This would be the case if simultaneously a) the Mexican municipal shares—i.e., share of migrants from municipality M living in county C—between counties were markedly different, b) the composition of the Mexican shares was correlated with the outcomes of interest, and c) the migration patterns between municipalities changed significantly during the period of study. To illustrate, assume that the people from northern Mexico had stronger networks in more conservative US counties and the people from southern Mexico, with

[12] We also argue that our initial shares (fraction of people from Mexican municipality M living in US county C) reflect historical linkages between US cities/counties and Mexican municipalities/states (Durand, 2016) and are not obviously endogenous to the changes we estimate.

[13] The potential for spatial correlation is the reason we leave out the entire CBSA, not only the county itself. In Appendix J we show that, while there is some spatial auto-correlation in the number of newcomers among counties (Moran's I of between .44 and .3), the correlation among CBSAs is significantly lower (Moran's I of between .21 and .18). Moreover, as Table 5 indicates, our results are robust to including a spatial lag.

a comparable population, had stronger networks in more liberal counties. Further, assume that the migration from northern Mexico increased during our period of study and migration from southern Mexico decreased. As a result, liberal counties would receive fewer Mexican migrants. This scenario represents the non-random exposure to shocks described by Borusyak and Hull (2020).

We conduct the following checks to provide evidence against both of these concerns in our main specification. First, we test for pre-trends by analyzing the association between the instrument and the lagged outcomes. We find a statistically significant correlation in only one lagged outcome, providing support for the parallel-trends assumption. Second, we test for differential trends by interacting key pre-period characteristics with period indicators. Our baseline results are, for the most part, robust to these controls; key regressors do not seem to have evolved along predicted unauthorized migration. Third, we implement the correction proposed by Borusyak and Hull (2020) to deal with possible non-random exposure. That is, we control for a constructed counterfactual instrument.[14] Controlling by this simulated variable is also useful to test whether the results are solely driven by the initial shares. Our main results are largely unaffected. All these results are displayed in Table 5.

Finally, we analyze the concentration of migrant networks by county. The predicted Mexican migrant composition in counties is not excessively concentrated. The top 50 sending municipalities account for a little over 30% of predicted migrants. On average, each of these top 50 municipalities provides only around 1.5% of the total predicted migrants per county, but have migrants living in over 560 counties. The average county has predicted migrants from around 95 municipalities. In Appendix L we calculate the Rotemberg weights, as suggested by Goldsmith-Pinkham et al. (2020). The top 17 Mexican municipalities account for only 30% of the positive weight in the instrument.

3.2 First Stage

The stability of the migration patterns results in a strong first stage. As Column 2 of Table 2 indicates, conditional on county fixed effects and state-by-period fixed effects, a 1 percentage point increase in the instrument is associated with a 1.16 percentage point increase in the observed share of newcomers. The F-stat of our instrument is 822.

For comparison purposes, Table 2 presents four instruments. Column 1 shows the results

[14] To obtain the simulated instrument, we average 2,000 instruments created by interacting 2,000 randomly permuted shifters with the original shares. To illustrate, from the total 585 migrants from Alvarado in the 2002–06 period, 9.2% lived in Los Angeles County, 7.5% in Ventura County, and 5.8% in Milwaukee. The shifter for each of these counties—created via the described LOO—in the period 2007–10 was 46, 39, and 26. In each simulation, we use instead any other of the over 300,000 shifters from that period—say 2,300 and 15.

for the least conservative instrument. This is almost an identical instrument to the leave-one-out; however, it leaves out only the county of interest rather than the CBSA. This results in the strongest first stage with an F-stat of 880. The instrument in Column 3 leaves the whole state out.

Column 4 displays the results of the push factor shift-share instrument. Despite exploiting variation in Mexican municipalities, the first stage is very strong. Throughout the rest of the paper, we use the leave-one-CBSA-out as our preferred specification, but also present the push factors instrument. Results are similar to the other two LOO instruments.

The Local Average Treatment Effect (LATEs) that these instruments provide is specific. Our estimand is the effect of flows of newcomers who migrate to places where they have networks. Our estimand is not the effect of random flows of recently arrived unauthorized Mexican migrants; rather, the migrants we study are those that settle in areas where people from their municipalities settled in the past. By construction, we are not capturing the effect of economically efficient migration, defined as migration towards locations where the real wage is highest. This is most clear with the push factor instrument. Emigrants pushed by adverse economic and social conditions probably rely on their networks even more.

Table 2: First stage

	(1) LOO, county out	(2) LOO, CBSA out	(3) LOO, state out	(4) Push factors
Newcomers, percent	1.116***	1.160***	1.315***	1.338***
population	(0.038)	(0.040)	(0.067)	(0.053)
Observations	8019	8019	8019	8019
F statistic	880	822	386	625
Mean of Dep. Var	0.463	0.463	0.463	0.463
Mean of Ind. Var	0.421	0.404	0.318	0.442

Column 1 displays the results for a leave-one-out (LOO) shift-share regressor that leaves the country itself out. Column 2 displays results for a LOO shift-share regressor that leaves the CBSA out. Column 3 displays results for a LOO shift-share regressor that leaves the state out. Column 4 displays results for a shift-share regressor that predicts yearly migration by municipality using push factors like poverty and homicide rates, economic activity and variation in temperature and precipitation. Standard errors clustered at the CBSA level. All estimations control for county and state-year fixed effects and are weighted by predicted population. Stars indicate *p<0.1,**p<0.05,***p<0.01

4 Main Results

In this section, we examine the impact of recent unauthorized Mexican migration on voting behavior. We then address two closely related questions: Can migrants affect the outcome of elections? Do migrants affect the policy delivered? The main finding is that flows of recent unauthorized migrants shift a county's vote share toward the right and prompt local spending consistent with the fiscal conservatism and the law-and-order policies of the Republican party.

4.1 Voting Behavior

Table 3 displays the estimated impacts of unauthorized migrant arrivals on Republican party vote share in House and presidential elections. We estimate midterm and presidential years separately for two reasons. (1) Midterm elections coincide with the end of our periods, whereas presidential elections occur two years later, which could capture a different timing of the effect. (2) Presidential elections are partially driven by presidential candidates, so they tend to be less about party differences (Campbell, 1987; Knight, 2017).

Throughout the article, we interpret effects in terms of mean flows of unauthorized migrants. Substantively, mean flows are quite small. In a county of 100,000 people, they translate into an average of 101 people annually. This quantity helps us to compare the political and policy behavior of a county in the presence of a mean flow of unauthorized migrants to the absence of that flow. Therefore, below the standard errors of the estimations, we report standardized coefficients and effects for a mean inflow ($\hat{\beta} * \bar{x}$). The standardized coefficients are useful for comparing magnitude across models. However, they largely capture cross-sectional variation, and counties are unlikely to move a standard deviation in inflows of unauthorized migrants. The impact of a mean inflow thus provides a more informative measure for policy.

The baseline OLS estimates, in Panel A, show that there is a statistically significant, positive relationship between unauthorized migration and Republican vote share. The coefficients present a pattern that is consistent with the IV estimates. The House midterm relationship is the largest (Columns 1 and 2). A one percentage point increase in unauthorized migrants is associated with 6.51 point increase in the share of votes that go to Republicans. Presidential year relationships are smaller in magnitude, both for the House of Representatives (Columns 3 and 4) and for the President (Columns 5 and 6). Finally, our weighted and unweighted estimates seldom differ statistically. We focus on population-weighted estimates throughout the remainder of the paper because these estimates are often more precise and robust than the unweighted estimates, and more informative about the effects on the country as a whole.

Panel B presents the LATEs of our preferred specification, the leave-one-CBSA-out shift-share (LOO CBSA) instrument which follows Tabellini (2020). In the House midterm elections, a mean flow of unauthorized migrants causes a 3.93 point increase in vote share for Republican candidates (Column 1, std coeff: 0.26). In presidential years, a mean flow of unauthorized migrants causes a 1.61 point increase in vote share for Republican House candidates (Column 3, std coeff: 0.10) and a 2.22 point increase for the Republican presidential candidate (Column 5, std coeff: 0.17).

The 2SLS estimates in Panel C are from the shift-share instrument that leverages shocks in Mexican municipalities. This LATE captures the impacts of migrants who arrive because of shocks in Mexico and the migrant network. Generally, the estimates from this instrument are statistically identical to those from the LOO CBSA instrument. A mean flow of migrants (moved by shocks and migrant networks) causes a 3.6 point increase in Republican vote share (Column 1, std coeff: 0.24). With this instrument, the weighted impacts in presidential years are also precise (Columns 3 and 5).

Table 3: Effects of arrival of unauthorized Mexican migrants on GOP vote shares (2010-20)

	House, Midterm		House, Pres year		President	
	(1) Weight	(2) Un-weight	(3) Weight	(4) Un-weight	(5) Weight	(6) Un-weight
A. OLS						
Newcomers, pct. pop.	6.51***	5.25***	2.82***	3.25***	3.48***	3.36***
	(0.87)	(0.70)	(1.06)	(0.65)	(0.62)	(0.44)
B. 2SLS, Loo-cbsa						
Newcomers, pct. pop.	8.49***	9.18***	3.49***	4.93***	4.81***	5.85***
	(1.03)	(1.05)	(1.21)	(0.94)	(0.69)	(0.68)
Std. Coefficient	0.26	0.23	0.10	0.12	0.17	0.17
$\hat{\beta} * \bar{x}$	3.93	2.52	1.61	1.35	2.22	1.60
C. 2SLS, push factors						
Newcomers, pct. pop.	7.77***	8.81***	3.29***	5.08***	5.09***	6.36***
	(1.14)	(1.19)	(1.27)	(1.03)	(0.70)	(0.72)
Std Coefficient	0.24	0.22	0.10	0.12	0.18	0.18
$\hat{\beta} * \bar{x}$	3.60	2.42	1.52	1.39	2.35	1.74
Observations	7995	7995	8015	8015	8019	8019
Dep. Var., Mean	48.16	61.70	47.24	63.79	46.05	61.70
Dep. Var., Sd	19.44	18.14	19.92	19.14	16.51	15.52

Dependent variables are share of Republican vote. Source: Dave Leip's United States Election Data. Newcomers are the number of new consular IDs per county per 4–year period as a proportion of predicted population as described in Section 3. Panel A displays the results for OLS estimator. Panel B displays results for a LOO shift-share regressor that leaves the CBSA out, our preferred specification. Panel C displays results for a shift-share regressor that predicts yearly migration by municipality using push factors like homicide rates, economic activity and variation in temperature. Standard errors clustered at the CBSA level. All estimations control for county and state-year fixed effects. *p<0.1,**p<0.05,***p<0.01

These effects are consistent with historical findings and findings from other countries receiving migrants. Mayda et al. (2022a) estimate that a one percentage point increase in the share of "low-skilled" migration raises the vote share for the Republican party, in all federal elections between 1990 and 2016, by 4.5 percentage points. There are several plausible reasons why our estimates are greater than theirs. First, the populations we study are different. As explained in the mechanisms section, the arrival of unauthorized migrants may have specific job market and demographic impacts. Second, unlike them, we analyze the role of flows, not stocks. Third, we focus on a historical period in which the Republican party has turned more anti-immigration.

Collectively, these results suggest that migrant flows driven by the existing migrant network prompt more conservative voting. In line with the literature (Barone et al., 2016; Edo et al., 2019; Mayda et al., 2022a; Tabellini, 2020), the LATEs we estimate are larger in magnitude than the OLS coefficient. Our hypothesis, again in line with the literature, is that migrants tend to move to counties that are more politically welcoming. [15]

For each analysis in the remainder of the paper, we present three sets of estimates. In Panel A, we show OLS estimates for a baseline comparison. Panel B displays second-stage estimates from the leave-one-out shift-share, and Panel C displays second-stage estimates from the push factors instrument. We use the reduced form estimates when we investigate robustness and heterogeneity.

4.2 Election Outcomes

Even though the impacts on Republican vote share are large, it is not clear from these estimates that average flows of migrants will alter the outcome of any House election or the composition of the House. It is possible that our mean effect is coming from already secure Republican counties, since the average vote share for Republicans across counties is already near 60%. To examine this question, we estimate the impact of unauthorized migrants on midterm House elections according to whether the Republican party won that county in the 2006 House election, the Republican vote share in the 2006 House election, and the closeness of the 2006 election. Figure 3 presents the effect of the interaction between the LOO instrument (in reduced form estimation) with an indicator of previous political behavior.

The first graph simply displays differential results based on whether the GOP won that county's vote in the 2006 House midterm election. The baseline category (in black) comprises counties where the Republicans won, and the blue line displays the differential effect of the inflow of predicted newcomers in counties where the Republican party lost. The interaction is not statistically significant. That is, the impact of the inflow of migrants is not statistically different in counties where the GOP lost. The second graph shows differential results by the Republican margin in the 2006 House midterm election. The baseline category (in black) comprises counties where the Republican party won by over 15 percentage points. The first blue line displays the differential effect of the predicted inflow of migrants in counties where the Republican party won by less than 15 percentage points. The second and third lines show

[15] Appendix M provides suggestive evidence consistent with this interpretation. Moreover, Cadena and Kovak (2016) show that "low-skilled" Mexican-born immigrants move at significantly higher rates in the face of economic shocks than similarly skilled (and even "high-skilled") native workers.

the differential effect in counties where Democrats won by 0 to 25 percentage points and by over 25 percentage points, respectively. No interaction is statistically significant, probably due to small sample sizes. In any case, these imprecise effects are small in magnitude and are not systematic. Namely, the estimated impact of predicted newcomers is smallest in Democratic-leaning counties (Democratic margin of 0-25) but largest in heavily Democratic ones (margin greater than 25). In the third graph, the omitted category consists of counties where either party won by more than 20 percentage points, a landslide, in 2006. In each successive group, the electoral margin narrows. Again, the interactions are not statistically significant, large, nor systematic. The differential impact in semi-competitive counties is 2.8 percentage points less than in safe seats, whereas the effect is 0.6 percentage points higher in competitive counties. Together, these results suggest that recent unauthorized Mexican migrants move electoral preferences to the right across the national geography, potentially deciding narrow races.

4.3 Policy Change

To explore whether the observed effects on federal elections reflect local conservative response, we study the impact of flows of newcomers on county-level public expenditures. Examining public spending gives us leverage on questions of interest. First, it allows us to explore whether the inflow of new unauthorized migrants creates a reduction in the provision of local public goods, in line with the predictions of Alesina et al. (1999). Second, we are able to explore whether the changes in public spending are consistent with a party that is more fiscally conservative, opposes redistribution, and focuses on law-and-order. Finally this exercise helps to connect preferences in national elections to changes in local policy.[16]

Table 4 presents the effects of inflows of recent newcomers on absolute and on relative expenditures. The former allows us to compare actual policy provision, but the estimates could be biased towards zero due to the budgeting heterogeneity.[17] The latter allows us to compare changes in relative preferences taking the fiscal heterogeneity as given.

The results present a pattern similar to that of the impact on voting. The OLS estimates provide a baseline that suggests a bias toward zero (Panel A), consistent with the expectation that migrants self-select into more politically welcoming eras. Second stage (Panels B and C) estimates are larger in magnitude, and most are precisely estimated.

Direct expenditure (Column 2) goes down in response to recent inflows of unauthorized

[16] While migration policy may reflect national or international political objectives (Camarena, 2022), individuals appear to hold their central governments accountable for the local migration dynamics (Kreibaum, 2016).

[17] Given that we exploit within period county variation, the measurement error in the fiscal variables is probably classical.

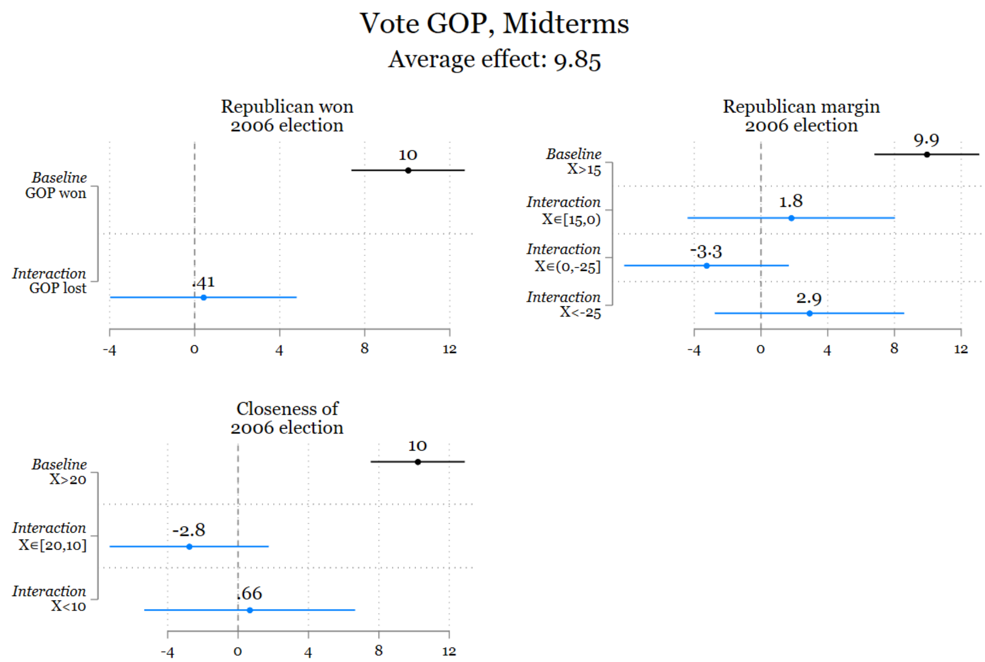

Figure 3: Effects of recent newcomers on the vote share for the Republican party in midterm elections by pre-period political behavior. The first graph presents the differential effect of predicted newcomers based on whether the Republican party won the county's vote in the 2006 House midterm election. The baseline category (black line) consists of counties where the Republican party won, and the blue line displays the interactive effect of the inflows of predicted migrants in counties where the Republicans lost. The first group comprises counties where the Republican party won by less than 15 (pp). The second picture presents the differential effect of predicted newcomers based on the margin of the Republican party in the 2006 House midterm election. The baseline category (black line) comprises counties where the Republican Party won by over 15 percentage points. The first group consists of counties where the Republican party won by less than 15 (pp). The second group consists of counties where the Democratic party won by less than 25 pp. The third group comprises counties where the Democratic party won by more than 25 pp. The third picture presents the differential effect of predicted newcomers based on the competitiveness of the 2006 House midterm election. The omitted category (black line) comprises counties where either party won by more than 20 pp. The first group consists of counties where either party won by between 20 and 10 pp. The second group consists of counties where the margin was less than 10 pp. Displayed are the coefficient of the interaction between the LOO instrument and the relevant dummy. Estimations are reduced form. The width of the line displays the 90% confidence interval.

migrants. This shift is consistent with conservative policy and a preference for less redistribution. The second stage estimates (Panel B) indicate that a mean flow of unauthorized migrants reduces direct expenditure per capita by 2% (Column 2, std coeff: -0.07).[18] Given that local governments often have balanced budget requirements, expenditures and revenues should move together (Kincaid, 2012; Tax Policy Center, 2021). Indeed, as Column 1 indicates, a mean inflow of unauthorized Mexican migrants reduces revenues per capita by 2% as well (Column 1, std coeff: -0.05).

It is hard to infer from these two estimates whether the conservative reaction indeed reflects a policy preference. An alternative explanation could be that both revenues and expenditures decrease as a result of an external, seemingly unrelated factor like a drop in intergovernmental transfers. In Appendix N we present the impact of unauthorized migration on the different components of local revenues. We find a decline in own source revenue, especially income tax, and no effect on intergovernmental transfers, which suggests an impact on local policy decisions.

As hypothesized by Alesina et al. (1999), this decrease in spending masks heterogeneity between categories. Migration inflows generate a reallocation across local public goods, away from productive expenditures. A mean flow of newcomers prompts a 3% reduction in spending per child on public education (log) (Column 3, std coeff: -0.10), and increases in police (2%) and judicial (8%) expenditures per capita.[19]. In response to new unauthorized migrants, local politicians limit spending on public education and invest in security. These allocations are consistent with the small government and law-and-order platform of the GOP.

Columns 6, 7, and 8 of Table 4 display the results for relative spending. Police spending as a share of direct expenditure increases by 0.23 percentage points (Column 7, std coeff: 0.15), and judicial expenditure as a share of direct expenditure increases by 0.15 percentage points (Column 8, std coeff: 0.21). That is, local governments explicitly decide to strengthen law-and-order.[20]

Given that the specific revenue and expenditure processes vary considerably within and between states, and even between expenditure categories (Fisher, 2016; Gordon et al., 2019; Martell and Greenwade, 2012; Tax Policy Center, 2021), our interpretation is that these re-

[18]Since we study only two periods (2007–10 and 2011–14), the mean flow we interpret is larger, 0.55 new unauthorized migrants as a percentage of predicted population.

[19]Since education is the largest budget category at the local level, its impact drives the effect on total spending.

[20]Since we analyze multiple public expenditure variables, we carry out a Holm correction for multiple hypothesis testing. With a 0.05 significance level, we can reject the null hypothesis of 4 of the 8 tests with a p-value of 0.009, less than Holm's benchmark of 0.01: police share, education (log per child), direct expenditures (log per capita), and judicial share. In the remaining analysis, we consider these four effects statistically significant.

Table 4: Public spending effects of arrival of unauthorized Mexican migrants (2012 and 2017)

	Expend (log pc 2010 USD)					Share of Dir Expend		
	(1) Revenue	(2) Direct exp	(3) Educ	(4) Police	(5) Judicial	(6) Educ	(7) Police	(8) Judicial
A. OLS								
Newcomers, pct. pop.	-0.02*	-0.02*	-0.03**	0.02	0.08	0.32	0.20*	0.13
	(0.01)	(0.01)	(0.01)	(0.02)	(0.06)	(0.42)	(0.12)	(0.09)
B. 2SLS LOO								
Newcomers, pct. pop.	-0.03**	-0.04***	-0.05***	0.04**	0.15**	0.23	0.42***	0.26***
	(0.01)	(0.02)	(0.02)	(0.02)	(0.07)	(0.61)	(0.14)	(0.10)
Std. Coefficient	-0.05	-0.07	-0.10	0.06	0.12	0.01	0.15	0.21
$\hat{\beta} * \bar{x}$	-0.02	-0.02	-0.03	0.02	0.08	0.13	0.23	0.15
C. 2SLS Push factors								
Newcomers, pct. pop.	-0.03**	-0.04**	-0.03**	0.04*	0.11	0.92	0.38**	0.21**
	(0.01)	(0.02)	(0.01)	(0.02)	(0.07)	(0.56)	(0.16)	(0.10)
Std. Coefficient	-0.05	-0.07	-0.07	0.05	0.08	0.05	0.14	0.17
$\hat{\beta} * \bar{x}$	-0.01	-0.02	-0.02	0.02	0.06	0.51	0.21	0.12
Observations	5338	5338	5328	5334	5266	5328	5334	5266
Dep. Var., Mean	1.57	1.53	1.96	-1.44	-2.95	40.93	5.45	1.41
Dep. Var., Sd	0.38	0.38	0.34	0.49	0.87	11.54	1.85	0.85
Ind. Var., Mean	0.55	0.55	0.55	0.55	0.55	0.55	0.55	0.55
Ind. Var., Sd	0.68	0.68	0.68	0.68	0.68	0.68	0.68	0.68
Inst. Loo, Mean	0.49	0.49	0.49	0.49	0.49	0.49	0.49	0.49
Inst. Loo, Sd	0.63	0.63	0.63	0.63	0.63	0.63	0.63	0.63

Dependent variables in columns 1–5 are in log 2010 dollars per capita, except education (column 3) which is per child (population under 19). Dependent variables in columns 6–8 are shares of total direct expenditures. Revenue includes taxes, intergovernmental revenue, current charges, and miscellaneous general revenue. Direct Expenditure includes spending on public education, policing, health, as well as other categories as described in section 3. Education expenditures include all public education expenditures of the county. Police expenditures include city police spending in a county as well as sheriff department spending and local incarceration at county jails. Judicial expenditure includes all county expenditures on the administration of justice including prosecutors, public defense, judges, court administration, and expenses related to the civil court system. Source: Annual Survey of State and Local Government Finances. Newcomers are the number of new consular IDs per county per 4-year period as a proportion of predicted population as described in Section 3. Source: SRE. Instruments are as described in Section 3. All regressions have period and county fixed effects. Standard errors are clustered at the CBSA level. Estimations weighted by predicted population. Stars indicate *p<0.1,**p<0.05,***p<0.01

sults are consistent with Republican fiscal policy in favor of smaller government and greater spending on law-and-order. Voters who shift toward Republican candidates for Congress probably also shift toward Republican down the ballot. This means more Republican candidates in city, county, school districts, and state positions. These politicians can make changes on the margin in the short run. The changes in education, police, and judiciary spending we identify probably reflect Republicans' collective efforts. To compare, Mayda et al. (2022b) find that a 1 percentage point increase in the population of "low-skilled" immigrants to the US between 1990 and 2010 reduced local per capita revenues and expenditures by 1.8%.[21] They do not find significant effects on education and law-and-order. As mentioned before, the political context, populations of study, and type of independent variable (flows vs. stocks) probably explain the differences with respect to our estimates.

Our findings on public spending are consistent with the ethnic heterogeneity/polarization (Alesina et al., 1999; Bazzi et al., 2019), compositional amenities (Card et al., 2012) and out-group bias (Ajzenman et al., 2021, 2022; Riek et al., 2006; Derenoncourt, 2022) mechanisms. Unauthorized migration causes divestment in education, the largest "productive expenditure" that local governments control, suggesting that residents may prefer to limit redistribution to the out-group. The effects on the relative investment in policing and the administration of justice could indicate an increased perception of threat and a desire to guarantee the power of the majority.

5 Robustness Checks

Our empirical strategy relies on the assumption that the observed effects in the variables of interest are solely due to the instrument via the endogenous variable. We assume that counties with more predicted migrants were not already in a different trend due to, for example, persistent impacts of the initial shares or the evolution of other observed or unobserved key variables. (Borusyak et al., 2022; Cunningham, 2021; Goldsmith-Pinkham et al., 2020).

We test this hypothesis in Table 5. First, in row B, we test for pre-trends by regressing the instrument on pre-period outcomes—lagged 12 years (3 periods). The values for the midterm elections are results in 1998, 2002, and 2006, for the presidential year elections in 2000, 2004, and 2008, and for the fiscal outcomes, values for 2002 and 2007.

Second, we test for differential trends by interacting several pre-period characteristics with period indicators in rows C–F. The intention is to explore whether the observed effect is being driven by the evolution of key pre-period characteristics rather than by our instrument.

[21] A 1 percentage point inflow of unauthorized Mexican migrants would correspond with a drop of 2.8% in per capita revenues and 4.1% in per capita direct expenditure.

Table 5: Robustness checks

	Midterms	Pres year		Log pc		Share of Expend	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	House	House	Pres	D. Exp	Educ	Police	Judicial
A. Reduced form, baseline							
Instrument	9.85***	4.04***	5.57***	-0.05***	-0.06***	0.49***	0.31***
	(1.22)	(1.37)	(0.77)	(0.02)	(0.02)	(0.17)	(0.12)
B. Lagged outcome (LO)							
Instrument	0.99	2.91	4.10***	-0.00	-0.02	-0.03	-0.02
	(2.01)	(2.04)	(0.56)	(0.02)	(0.01)	(0.20)	(0.11)
C. Mex non-citizen, sh							
Instrument	9.85***	8.09***	8.71***	-0.04*	-0.06**	0.42**	0.33**
	(1.36)	(2.31)	(1.07)	(0.03)	(0.03)	(0.19)	(0.15)
D. Hispanics, sh							
Instrument	10.33***	4.91***	6.93***	-0.04**	-0.06***	0.48***	0.29**
	(1.33)	(1.77)	(0.75)	(0.02)	(0.02)	(0.18)	(0.12)
E. Adult HS completion							
Instrument	11.19***	5.64***	7.15***	-0.05***	-0.06***	0.51***	0.32***
	(1.25)	(1.31)	(0.57)	(0.02)	(0.02)	(0.17)	(0.12)
F. China shock							
Instrument	8.41***	2.71*	3.90***	-0.05***	-0.07***	0.53***	0.33***
	(1.35)	(1.47)	(0.76)	(0.02)	(0.02)	(0.16)	(0.12)
G. Simulated instrument							
Instrument	9.84***	7.94*	7.59***	-0.05	-0.13***	0.43*	0.49***
	(2.51)	(4.51)	(1.45)	(0.05)	(0.04)	(0.26)	(0.15)
H. Spatial lag							
Instrument	7.96***	4.22*	4.33***	-0.05**	-0.05	0.42***	0.25*
	(1.49)	(2.35)	(1.13)	(0.02)	(0.03)	(0.16)	(0.13)
I. Stock Mex foreign							
Instrument	10.03***	4.67***	5.66***	-0.05***	-0.06***	0.50***	0.31***
	(1.25)	(1.40)	(0.79)	(0.02)	(0.02)	(0.16)	(0.11)
J. Stock Hispanics							
Instrument	9.07***	2.50*	5.23***	-0.05**	-0.06**	0.57***	0.33**
	(1.18)	(1.39)	(0.78)	(0.02)	(0.02)	(0.20)	(0.14)
K. No-outliers							
Instrument	11.17***	4.24**	6.64***	-0.06**	-0.06**	0.64***	0.32*
	(1.37)	(1.77)	(0.84)	(0.03)	(0.03)	(0.21)	(0.17)
L. No pop weights							
Instrument	10.69***	5.74***	6.81***	-0.04	-0.06***	0.56***	0.24***
	(1.27)	(1.08)	(0.76)	(0.02)	(0.02)	(0.19)	(0.09)
*M. County-group * period FE*							
Instrument	10.11***	6.03***	6.27***	-0.09**	-0.09***	0.64*	0.28**
	(1.45)	(1.17)	(0.82)	(0.03)	(0.03)	(0.33)	(0.13)

Dependent variables in columns 1–3 are the vote share for Republicans in different federal elections. Dependent variables in column 4–5 are the log of per capita (per child population in column 4) expenditure. Columns 6–7 are the share of direct expenditure. All estimations are reduced form. Sources: Dave Leip's US Election Data; Annual Survey of State and Local Government Finances; US Census; USDA's Economic Research Service; Peter K. Schott's Data; County Business Patterns; ACS 5 from the Social Explorer; Acemoglu et al. (2016). and QCEW. Panel 1 is reduced form estimation. Panel 2 estimates effect on lagged dependent variables. Panel 3 controls for pre-2007 features interacted with period dummies. Panel 4 includes simulated instrument following Borusyak and Hull (2020). Panel 5 controls for the spatial lag of the instrument (values of neighboring counties). Panel 6 controls for the stock of Mexican non-citizens and Hispanics at the beginning of period. Panel 7 excludes outliers, does not use predicted population weights, and instead of state-period fixed effects uses politically similar counties-period fixed effects. Standard errors clustered at CBSA level. Estimations control for county and state-period fixed effects, except for row M. Estimations weighted by predicted population, except row L. Stars indicate *p<0.1,**p<0.05,***p<0.01

Row C, in particular, controls by the share of Mexican population without US citizenship in 2000, obtained from the US Census, interacted with period dummies. This control aims to model the evolution of unauthorized Mexicans given the stock of Mexican-born residents in 2000. Conditioning on such projection does not statistically alter our results. Panels D, E, and F condition on the share of Hispanics in 2000, the rate of high school completion among adults in 2000, and exposure to the China shock in 2006, constructed with Peter K. Schott's Data, County Business Patterns and Acemoglu et al. (2016) replication files. Neither of these three variables alters the magnitude or significance of our results —except again for vote shares during presidential years.

Third, we explore whether migration exposure was non-random. Non-random exposure would occur if migrants from certain Mexican municipalities had simultaneously sorted into politically biased counties and had migrated at systematically different rates over the period of study. In row G, we implement the correction proposed by Borusyak and Hull (2020). We construct a counterfactual instrument by taking the average of 2,000 simulated instrument shifters, created by multiplying the original shares by each of 2,000 permutations of the LOO shifters from other county-municipality dyads in the same period. The results remain largely significant.

To rule out the possibility of spatial effects and spillovers, row H controls for the spatial lag—an average of neighboring counties—of the LOO instrument. Most estimates are robust to this control. We also test for a different link between migration and political outcomes. To disentangle the role of stocks versus flows, row G explicitly conditions on the estimated share of residents who were born in Mexico the year before our periods started, obtained from the Social Explorer's ACS 5 2005–09, 2009–13, and 2013–17. While this variable theoretically captures both authorized and unauthorized migrants (and is noisier for smaller counties), it correlates strongly with the instrument (ρ or around 0.86). Conditioning on the share of Mexican-born residents does not change the estimates. Row J uses a similar control, the share of residents who identified as Hispanics, coming from the U.S. Census Bureau. Finally, Rows K–M present estimates excluding outliers (percentiles 1 and 99 of the predicted migration distribution), without using predicted population weights and exploiting variation within groups of politically similar counties (in the state) following Bazzi et al. (2021). The estimates remain consistent.

Using both the instrument and the lagged instrument, as recommended by Jaeger et al. (2018), is intended to explore whether there is a dynamic adjustment in the outcomes of interest in the presence of highly serially correlated instruments, like our setting. Their canonical example pertains to the impact of migration across decades (not years) in labor markets (not elections or budgets). The stability of our results across specifications, and their consistency with the literature, hints at a lack of dynamic adjustment. However, we implement the Jaeger et al. (2018) technique in Appendix O. Since we need a measure of the lagged instrument, this correction requires us to drop the first period and lose significant power. For electoral outcomes, that implies analyzing just 2010–14 and 2015–18, and for the public spending results of just the 2017 fiscal year, making the estimate merely a cross-section. Unsurprisingly, the results are inconsistent and do not reflect a dynamic pattern.

In Appendix P, we estimate a similar correction to the one proposed by Adão et al. (2019) to account for a potential correlation of the residuals between counties with comparable initial shares. Our confidence intervals are largely unchanged by these methods.

6 Mechanisms

There are several explanations consistent with the main findings on conservative electoral and policy responses documented above. Following the literature, here we study three sets of outcomes to shed light on underlying mechanisms.

In subsection one, we explore the effect of flows of unauthorized migrants on formal employment and wages. The goal is to identify whether migrants have displaced existing workers and pushed down wages in specific industries that typically employ unauthorized migrants (i.e., partial equilibrium effects). The literature shows that Mexican migration generates either zero or small general equilibrium effects, since migrants contribute to economic activity beyond undercutting workers in partial equilibrium Blau and Mackie (2017); Clemens et al. (2018); Hanson (2009); Monras (2020). Therefore, in subsection two, we study aggregated/general economic indicators (i.e., general equilibrium effects). We focus on GDP per capita, median household income, unemployment, and poverty. In subsection three, we examine demographic changes and moral values. Our goal is to estimate if migrants affect the demographic composition of the county, via residential sorting and internal migration, as well as the cultural preferences of residents. For demographic variables, we examine the effect on out-migration and adult population, both total and Hispanic, Black, and White separately. For moral values, we study the relative importance of universalist versus communal values. This variable, obtained from Enke (2020) replication files, aims to capture people's beliefs about the relative moral emphasis on the in-group vs. the out-group. Those inclined toward universalism emphasize equal treatment, regardless of relationship, whereas those inclined toward communalism emphasize loyalty to members of the in-group. All the variables are summarized in Appendix H.

Importantly, there are two theories we cannot test due to a lack of data. While we have data on objective economic (and crime) indicators, we do not have systematic information on people's perceptions. Thus, we cannot directly test the threat hypothesis, whereby established residents are hostile to migrants due to a perceived threat or danger Ajzenman et al. (2021). Our setting also does not allow us to measure the role of political entrepreneurs. Since the flow of unauthorized migrants affects vote shares and attitudes simultaneously, we cannot determine whether the conservative reaction is being fueled by anti-immigrant discourse—as documented by Djourelova (2023).

6.1 Employment and Wages by Sector

Labor market theories suggest that migrants can decrease employment and wages among similarly skilled natives (Peri and Sparber, 2009; Blau and Mackie, 2017; Borjas and Edo, 2021). Politicians may in turn promise anti-migrant policy to attract those who lost in the labor market. To test if flows of unauthorized migrants generate economic losses in formal employment or wages, which could explain the conservative shift, we use the Quarterly Census of Employment and Wages (QCEW). This source reports the annual average employment and weekly wages for multiple sectors and super-sectors across the US. We examine total average annual employment and wages and break out the super-sectors of hospitality and leisure (H&L) and agriculture (which also includes forestry, fishing, and hunting). The employment variables are measured in (log) per working-age population (ages 15–64, US Census) and the wages correspond to 2010 dollars.[22]. The QCEW estimates are based on states' unemployment insurance data and surveys from employers. They explicitly exclude "self-employed workers, most agricultural workers on small farms...some domestic workers" (US Bureau of Labor Statistics, 2022). Hence, our working assumption is that this is formal employment.

Columns 1-5 of Table 6 present the results on formal employment. Inflows of unauthorized Mexican migrants have an imprecisely estimated zero effect on total employment. On average, employment per working-age people is unmoved by the arrival of migrants. This null effect, however, masks heterogeneity, as some sectors observe a decrease and others an increase. A mean inflow of unauthorized migrants reduces employment in the construction industry by 2% (Panel B, column 2, std coeff: -0.07) and by 1% in H&L (Panel B, column 4, std coeff: -0.03). At the same time, mean migrant flows increase employment in manufacturing by 3% (Panel B, Column 3) leaving the main effect on total employment a precise zero (Panel B, column 1). We also estimate a decline in agricultural jobs, but it is poorly estimated, probably due to the small sample size and the exclusion of (formal) agricultural workers from the dataset.

[22]Since they are normally distributed, we study them in levels.

Table 6: Effect of arrival of unauthorized Mexican migrants on employment among working age population and weekly wages (2010-19)

	Employment, (log per working age pop)					Weekly Wages (2010 USD)				
	(1) Total	(2) Constr	(3) Manufact	(4) Hosp and leis	(5) Agric	(6) Total	(7) Constr	(8) Manufact	(9) Hosp and leis	(10) Agric
A. OLS										
Newcomers, pct pop.	0.01	-0.03*	0.07***	-0.01**	0.03	3.98	-14.35	12.37	-4.83	-18.30
	(0.01)	(0.02)	(0.02)	(0.01)	(0.06)	(14.27)	(9.58)	(24.00)	(4.45)	(12.19)
B. 2SLS Loo										
Newcomers, pct pop.	-0.00	-0.05***	0.08***	-0.02**	-0.09	-1.78	-22.58*	16.02	-8.06	-42.74**
	(0.01)	(0.02)	(0.02)	(0.01)	(0.08)	(18.66)	(12.82)	(33.34)	(6.44)	(19.38)
Std. Coefficient	-0.00	-0.07	0.06	-0.03	-0.04	-0.00	-0.06	0.03	-0.04	-0.14
$\hat{\beta} * \bar{x}$	-0.00	-0.02	0.03	-0.01	-0.05	-0.82	-10.47	7.43	-3.73	-22.07
C. 2SLS push factors										
Newcomers, pct pop.	-0.00	-0.04*	0.09***	-0.03***	-0.05	-13.60	-26.18***	-11.10	-9.89*	-41.48**
	(0.01)	(0.02)	(0.02)	(0.01)	(0.09)	(12.69)	(9.72)	(21.02)	(5.89)	(18.90)
Std. Coefficient	-0.00	-0.06	0.07	-0.04	-0.02	-0.03	-0.07	-0.02	-0.05	-0.13
$\hat{\beta} * \bar{x}$	-0.00	-0.02	0.04	-0.01	-0.02	-6.29	-12.14	-5.15	-4.57	-21.42
Observations	8003	7388	7376	7906	4117	8003	7388	7376	7906	4117
Dep. Var., Mean	-0.67	-3.62	-3.07	-2.75	-6.56	872.40	992.86	1120.65	365.50	611.14
Dep. Var., Sd	0.34	0.44	0.73	0.43	1.49	260.60	223.12	354.91	119.55	199.50
Ind. Var., Mean	0.46	0.46	0.46	0.46	0.52	0.46	0.46	0.46	0.46	0.52
Ind. Var., Sd	0.59	0.59	0.59	0.59	0.63	0.59	0.59	0.59	0.59	0.63
Inst. Loo, Mean	0.40	0.41	0.41	0.40	0.47	0.40	0.41	0.41	0.40	0.47
Inst. Loo, Sd	0.55	0.55	0.55	0.55	0.59	0.55	0.55	0.55	0.55	0.59

Dependent variables in columns 1–5 are the log of average annual employment divided by working age population. Dependent variables in columns 6–10 are the annual average weekly wages in 2010 USD. Sources: Quarterly Census of Employment and Wages. Newcomers are the number of new consular IDs per county per 4-year period as a proportion of predicted population as described in Section 3. Source: SRE. Instrument is as described in Section 3. Standard errors clustered at the CBSA level. All estimations control for county and state-period fixed effects. Estimations weighted by predicted population. Stars indicate *p<0.1,**p<0.05,***p<0.01

These findings indicate a reallocation of jobs, away from construction and H&L to manufacturing. Construction and H&L are two of the industries with the highest estimated concentration of unauthorized migrants (New American Economy, 2021; Passel and Cohn, 2015; Svajlenka, 2020). Day labor in construction is often readily available to Mexican newcomers. Contingent work has low barriers to entry, and Mexican communities often use informal organizations to facilitate day labor, which is disproportionately in construction (Valenzuela, 2003). In contrast, while it employs an estimated large share of unauthorized migrants, manufacturing is an industry that is likely to advantage those in formal employment. Hence, our interpretation is that, since their reservation wage is lower and their outside options are worse, unauthorized migrants are more willing to accept informal jobs (Kossoudji and Cobb-Clark, 2002; Ortega and Hsin, 2022), which are more common in the sectors of construction and H&L. While our results are consistent with the literature and the dynamic of the construction sector (Peri and Sparber, 2009), our data allow us to better observe shifts in the demand for informal work in different sectors.

Columns 6–10 of Table 6 present the results on wages. Again, we observe a small negative yet imprecisely estimated effect on total wages: the average worker, either in the formal or informal sector, does not earn less as a result of the arrival of unauthorized newcomers. We do, nevertheless, observe an impact on construction and agriculture.

A mean flow of migrants drives construction wages down by \$10.47 weekly (Panel C, Column 7, std coeff: -0.06) and agricultural wages down by \$22.07 weekly (Panel B, Column 10, std coeff: -0.14). Agriculture is the industry that employs the highest number of unauthorized Mexican migrants as a share of total employment, so the steep decline in wages does reflect an increase in the supply of laborers (New American Economy, 2021; Passel and Cohn, 2015; Svajlenka, 2020).

Economic theory would suggest that wages and employment should move in opposite directions. Our interpretation, which reconciles the simultaneous decline in wages and formal employment in construction and H&L, is that inflows of unauthorized migrants increase the total labor supply but decrease the formal labor supply.

Together, these findings are consistent with the literature on the impact of unauthorized migrants on labor market outcomes. Scholars generally find small reductions in wages and employment, often limited to a few immigrant-intensive sectors (Hanson, 2009; Monras, 2020; Blau and Mackie, 2017). The employment decreases in construction, and H&L are comparable to the ones observed with other global flows, like the drop in manufacturing employment due to the China shock (Autor et al., 2013).[23] This suggests that there are some

[23] Going from percentile 25 to percentile 75 of exposure to the China shock reduced employment in manufacturing by 4.5 percent between 2000 and 2007.

economic losers in counties that receive unauthorized migrant flows, and at first glance, these individuals are not compensated accordingly. Those with economic losses may well account for some voters punishing pro-immigrant politicians. The effects on informal employment are unclear, as there is no reliable data at the geographic and temporal level of this study.

6.2 General Economic Effects

In this section, we examine whether migration inflows affect other general equilibrium indicators, like income levels. We then extend this analysis by looking at the income distribution.

We start by estimating the effects on poverty, a potential consequence of job loss, and a key driver of economic grievance (Hopkins et al., 2023). Column 1 of Table 7 displays the effects of migration inflows on the natural logarithm of people in poverty in the county—-obtained from 2011, 2015, and 2019 values of the Small Area Income and Poverty Estimates (SAIPE) Program. As Panels B and C indicate, we identify a marginal increase in the number of people in poverty. A mean inflow of recent newcomers, estimated with either instrument, raises the number of impoverished people by 2% (Panels B and C, Column 1, std coeff: 0.02).

It is unclear to what extent SAIPE's poverty estimates include newcomers themselves, as they are calculated with data from ACS, tax returns, the previous Census, and SNAP (Supplemental Nutrition Assistance Program) beneficiaries. Hence, to approximate the effect of migration inflows on poverty among US citizens, the outcome of Column 2 of Table 7 is the natural logarithm number of SNAP recipients—also obtained from SAIPE. SNAP has the virtue of being among the most responsive federal entitlement programs, and it is unavailable to unauthorized migrants. Participation among non-citizens is minimal, and since the arrivals we are studying are from the previous four years, it is unlikely that they have US-born citizen children who qualify.[24] A disadvantage, however, is that participation is voluntary, so it does not fully capture poverty among citizens. A mean flow of unauthorized migrants increases the number of SNAP recipients in the county by 1% (Panel B, Column 2, std coeff: 0.01) with the LOO instrument—the effect is not statistically significant with the push factor instrument. This result suggests that the marginal increase in poverty is not entirely explained by newcomers themselves.

Column 3 of Table 7 estimates the impact of newcomers on the county-level poverty rate. A mean inflow of recent newcomers, estimated with either instrument, raises the share of impoverished people by 0.7 percentage points (Panels B and C, Column 3, std coeff:

[24] U.S. Department of Agriculture Food and Nutrition Service. Supplemental Nutrition Assistance Program: Guidance on Non-Citizen Eligibility. Found on the internet at https://www.nilc.org/wp-content/uploads/2019/05/Non-Citizen_Guidance_063011.pdf

Table 7: Socioeconomic effects of arrival of unauthorized Mexican migrants (2010-19)

	People in poverty (log)		County Economy (rate)		County Economy (log)	
	(1) People in poverty	(2) SNAP recipients	(3) Poverty rate	(4) Unemployment rate	(5) GDP per capita	(6) Median household income
A. OLS						
Newcomers, pct. pop.	0.04***	0.01	1.17***	-0.03	-0.01	-0.02
	(0.01)	(0.02)	(0.22)	(0.15)	(0.01)	(0.01)
B. 2SLS Loo						
Newcomers, pct. pop.	0.07***	0.03*	1.59***	0.18	-0.03	-0.02*
	(0.01)	(0.02)	(0.26)	(0.18)	(0.02)	(0.01)
Std. Coefficient	0.02	0.01	0.18	0.04	-0.04	-0.05
$\hat{\beta} * \bar{x}$	0.03	0.01	0.73	0.08	-0.01	-0.01
C. 2SLS push factors						
Newcomers, pct. pop.	0.07***	0.01	1.56***	0.06	-0.04**	-0.03**
	(0.01)	(0.02)	(0.27)	(0.19)	(0.02)	(0.01)
Std. Coefficient	0.02	0.00	0.17	0.01	-0.05	-0.06
$\hat{\beta} * \bar{x}$	0.03	0.00	0.72	0.03	-0.02	-0.01
Observations	8019	8019	8019	8019	7857	8019
Dep. Var., Mean	10.89	10.81	14.36	6.02	3.88	10.88
Dep. Var., Sd	1.63	1.65	5.31	2.83	0.44	0.26
Ind. Var., Mean	0.46	0.46	0.46	0.46	0.47	0.46
Ind. Var., Sd	0.59	0.59	0.59	0.59	0.59	0.59
Inst., Mean	0.40	0.40	0.40	0.40	0.41	0.40
Inst., Sd	0.55	0.55	0.55	0.55	0.55	0.55

Dependent variables in columns 1–2 are the log of poor people and log of SNAP beneficiaries. Dependent variables in columns 3–4 are poverty rate and unemployment rate. Dependent variables in columns 5–6 are the log of GDP per capita (in 2012 USD) and median household income (in 2010 USD). Sources: Small Area Income and Poverty Estimates (SAIPE) Program; US Department of Commerce: Bureau of Economic Analysis. Newcomers are the number of new consular IDs per county per 4-year period as a proportion of predicted population as described in Section 3. Source: SRE. Instrument is as described in Section 3. Standard errors clustered at the CBSA level. All estimations control for county and state-period fixed effects. Estimations weighted by predicted population. Stars indicate *p<0.1,**p<0.05,***p<0.01

0.18 and 0.17). As evidenced by the magnitude of the standardized coefficient, this effect is significantly larger than that of the number of poor people.[25] Flows of unauthorized migrants raise relative poverty an order of magnitude more than absolute poverty. One potential explanation for this divergence is residential sorting and demographic change. As the next section shows, relatively wealthier residents leave their counties of residence in response to the arrival of unauthorized migrants.

Column 4 of Table 7 displays the impact on the unemployment rate—-obtained from the Local Area Unemployment Statistics (LAUS) program. In contrast with employment, the unemployment rate captures the formal and informal sectors better as it measures people from the labor force who are jobless and are actively looking for jobs. Consistent with the impact on average wages, inflows of migrants have a small, positive, and statisitically insignificant effect on unemployment.

Column 5 of Table 7 presents the impact on (log) County GDP per capita—obtained from the Regional Economic Accounts of the Department of Commerce's Bureau of Economic Analysis. A mean inflow of recent newcomers, estimated with the push factor instrument, decreases GDP per capita by 4% (Panel C, Column 3, std coeff: 0.04). The effect is smaller (3%), but imprecisely estimated with the LOO instrument. Our interpretation is that this decline reflects two distinct forces. (1) Given that we do not identify a statistically significant effect on average wages, unemployment, and formal employment, our interpretation is that the decline in GDP per capita reflects, at least partially, a reduction in formal economic activity, as opposed to a reduction in economic activity in general. (2) As the effects on relative vs. absolute poverty indicate, the reduction in formal economic activity is probably caused, at least partially, by the emigration of relatively wealthier residents.

Column 6 of Table 7 shows the impact on (log) median household income—obtained from SAIPE. A mean inflow of recent newcomers decreases median household income by 1% (Panel B and C, Column 6, std coeff: 0.05 and 0.06, respectively). The effect largely mirrors that of GDP.

Together, these estimates depict three complementary results. In response to the inflow of unauthorized migrants, people at the bottom of the income distribution become marginally poorer, relatively wealthier residents migrate, and formal economic activity declines. These results are consistent with the conservative electoral and policy responses documented. However, it is not possible to determine to what extent they are the cause or the consequence of conservative policy. A reduction in total public expenditure at the local level could explain

[25] Comparing the regression coefficients as a share of the means of the dependent variables renders a similar conclusion. The effect on people in poverty is 0.006 of the mean, whereas the effect on the poverty rate is 0.10.

the rise in the number of poor people, but could also be explained by rising poverty.

6.3 Demographic Changes and Values

Other non-economic hypotheses for the observed conservative reaction are that the population changes in response to unauthorized migrant arrivals or that their policy preferences and values shift. Simply by virtue of their otherness, the arrival of recent unauthorized Mexican migrants could trigger exclusionary attitudes from US citizens or established residents. These attitudes could be reflected either in new opinions or values or, more profoundly, in the decision to switch residence. The resulting conservative reaction would be the result of the emigration of relatively left-leaning voters and/or a change in preferences for redistribution.

To study population changes, we rely on US Census data. We examine whether the (log) adult population, adult White population, adult Hispanic population, and adult Black population change the year after the end of our periods. The US Census systematizes data from the American Community Survey on county-to-county demographic flows. Thus, we construct out-migration rates by dividing the number of out-migrants by county population, using the data from 2007–11, 2011–15, and 2015–19. To examine moral values, we use the county-level index of the relative importance of universalist values versus communal values created by Enke (2020) from YourMorals.org. The index is available for 2,263 counties for the years 2007–10, 2011–14, and 2015–18. To create it, Enke standardized and scaled the counties' average values by their signal-to-noise ratio.

Columns 1 to 4 of Table 8 display the effects on adult population. In short, we observe a decline in total adult population, driven by White population, and an increase in both Black and Hispanic population. A mean flow of unauthorized migrants causes a 2% decrease in adult county population (Panel B, Column 1, std coeff: -0.01). In line with White flight and segregation, this decline is mostly explained by adult White population, which is reduced by 1% (Panel B, Column 3, std coeff: -0.01). In contrast, Hispanic and Black populations increase by 4% and 2%, respectively (Columns 2 and 4, std coeff: 0.02 and 0.01).[26] Column 5 shows that the population decline is attributable to out-migration. A mean flow of unauthorized migrants causes an increase of 1.58 out-migrants per 1,000 inhabitants (Panel B, Column 2, std coeff: 0.05). Hence, the arrival of recent newcomers generates residential sorting and demographic change, decreasing the population of the White majority and increasing the population of the two largest minorities. These results provide further evidence that relatively wealthier residents move out of their communities, bringing both median income and GDP per capita down.

[26] Since ethnic and racial categories are not mutually exclusive and change over time, the effects on the different groups do not necessarily add up to the effect on total adult population

Table 8: Effect of arrival of unauthorized Mexican migrants on values and demographic composition (2010-19)

	Adult Pop (log)				Per capita	Relative importance
	(1)	(2)	(3)	(4)	(5)	(6)
	Total	Hispanic	White	Black	Out migration	Universalist values
A. OLS						
Newcomers, pct. pop.	-0.03***	0.06***	-0.02***	0.02	1.26**	-0.09**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.58)	(0.04)
B. 2SLS Loo						
Newcomers, pct. pop.	-0.03***	0.08***	-0.03***	0.04**	1.58***	-0.13***
	(0.01)	(0.01)	(0.01)	(0.02)	(0.61)	(0.04)
Std. Coefficient	-0.01	0.02	-0.01	0.01	0.05	-0.16
$\hat{\beta} * \bar{x}$	-0.02	0.04	-0.01	0.02	0.73	-0.06
C. 2SLS push factor						
Newcomers, pct. pop.	-0.03***	0.09***	-0.02**	0.06***	1.34**	-0.16***
	(0.01)	(0.01)	(0.01)	(0.02)	(0.60)	(0.04)
Std. Coefficient	-0.01	0.02	-0.01	0.01	0.05	-0.19
$\hat{\beta} * \bar{x}$	-0.01	0.04	-0.01	0.03	0.62	-0.08
Observations	8019	8019	8019	8009	8017	5712
Dep. Var., Mean	12.62	10.22	12.36	10.01	55.16	0.15
Dep. Var., Sd	1.59	2.43	1.52	2.34	17.00	0.50
Ind. Var., Mean	0.46	0.46	0.46	0.46	0.46	0.47
Ind. Var., Sd	0.59	0.59	0.59	0.59	0.59	0.60
Inst. Loo, Mean	0.40	0.40	0.40	0.40	0.40	0.41
Inst. Loo, Sd	0.55	0.55	0.55	0.55	0.55	0.56

The dependent variable in columns 1-4 are the log of adult total county population, adult Hispanic population, adult White population and adult Black population. The dependent variable in column 5 is out-migration, calculated as the number of out-migrants (in thousands) divided by county population. The dependent variable in column 6 is the average relative importance of universalist values, taken from by Enke (2020). Sources: Enke, (2020); US Census Bureau: Population Division and Small Area; US Census Bureau: 2007-2011, 2011-2015, and 2015-2019 American Community Surveys 5. Newcomers are the number of new consular IDs per county per 4-year period as a proportion of predicted population as described in Section 3. Source: SRE. Instrument is as described in Section 3. Standard errors clustered at the CBSA level. All estimations control for county and state-period fixed effects. Estimations weighted by predicted population. Stars indicate *p<0.1,**p<0.05,***p<0.01

The final column in Table 8 explores the impact of unauthorized migrants on universalist (as compared to communal) values. We study individuals' universalist values to capture preferences for redistribution and openness to the out-group. Universalist values imply that one is concerned equally with the welfare of all individuals, whether they are known or not. By contrast, people with more communal values assign a greater weight to the welfare of in-group members relative to out-group members. We find that counties become less universalist in response to the arrival of new unauthorized migrants. A mean flow of unauthorized migrants shifts counties 0.06 standardized units toward less universalist, i.e., more communal (Panel B, Column 5, std coeff: -0.16). This result is the most direct indication that some of the shift to the political right occurs because migrants trigger anti-out-group bias and preferences for less redistribution. Although this evidence is based on a smaller subset of counties, the impact is large. The change toward more communal values is consistent with theories that hinge on out-group bias. Ethnic heterogeneity breaks down trust, makes coordination more difficult, and reduces people's interest in universal redistribution (Alesina et al., 1999).

An additional test would be to observe whether citizens change their ideology or partisan affiliation in response to unauthorized newcomers. The Gallup Daily tracker is, to the best of our knowledge, the only dataset that measures ideology in all US counties for our period of analysis. However, we do not have access to the most disaggregated version. Therefore, Appendix R estimates the impact of recent newcomers on ideology and partisan affiliation using the Gallup Daily version aggregated at the Metropolitan Statistical Area (MSA) level and the Cooperative Congressional Election Study (CCES)—currently known as the Cooperative Election Study (CES)—which covers only a subset of counties. There is imprecise evidence that people are becoming more conservative or identify more with the Republican party because of unauthorized migration. However, these results are not well-powered, as they rely on variation from fewer observations.

To explore the relationship between the different mechanisms at play, we estimate the differential impact of recent newcomers on universalism and out-migration according to whether the county had an above-median change in the poverty rate during our period of analysis (2011–2019). That is, we average the two changes in the poverty rate (2011–2015 and 2015–2019) and divide counties based on whether they have an above or below median value. Counties with above-median changes in the poverty rate are likely to cultivate more intense economic grievances. The comparison, therefore, allows us to test the link between economic shocks and cultural/moral impacts. If the conservative response is mainly driven by economic factors, counties with relatively better economic conditions should observe more temperate effects on out-group bias and residential sorting.

Table 9: Effects of the arrival of unauthorized Mexican migrants on values and out-migration (2010-19), by changes in poverty

	(1) Universalist values	(2) Out migration
A. OLS		
Newcomers, pct. pop.	-0.03	1.30*
	(0.04)	(0.75)
Above median	-0.20**	-0.48
poverty change × Newcomers, pct. pop.	(0.09)	(1.39)
B. Reduced form Loo		
Newcomers, pct. pop.	-0.13**	1.94**
	(0.06)	(0.92)
Above median	-0.12	-0.50
poverty change × Newcomers, pct. pop.	(0.12)	(1.94)
C. Reduced form push factor		
Newcomers, pct. pop.	-0.21***	1.71*
	(0.07)	(1.03)
Above median	-0.08	0.01
poverty change × Newcomers, pct. pop.	(0.14)	(2.27)
Observations	5709	8014
Dep. Var., Mean	0.21	53.88
Dep. Var., Sd	0.48	16.52
Dep. Var. above, Mean	0.08	56.98
Dep. Var. above, Sd	0.50	17.59
Inst. Loo, Mean	0.54	0.54
Inst. Loo, Sd	0.62	0.62
Inst. Loo. above, Mean	0.23	0.23
Inst. Loo. above, Sd	0.39	0.39

The dependent variable in column 1 is the average relative importance of universalist values, taken from Enke (2020). The dependent variable in column 2 is out-migration, calculated as the number of out-migrants (in thousands) divided by county population. Panels B and C are reduced form. We interact the corresponding instruments and fixed effects with an indicator of whether the county had an above-median relative change in the number of poor people during the three periods (2011–2015–2019). Sources: Enke, (2020); US Census Bureau: Population Division and Small Area; US Census Bureau: 2007-2011, 2011-2015, and 2015-2019 American Community Surveys 5; QCEW; SAIPE. Regressions are reduced form. Newcomers are the number of new consular IDs per county per 4-year period as a proportion of predicted population as described in Section 3. Source: SRE. Instrument is as described in Section 3. Standard errors clustered at the CBSA level. All estimations control for county and state-period fixed effects. Estimations weighted by predicted population. Stars indicate *p<0.1,**p<0.05,***p<0.01

Economic grievance seems to be connected to out-group bias, but not to residential sorting. As Table 9 shows, counties that have above-median changes in poverty rates tend to have more pronounced effects (imprecisely estimated) on universalist values, yet more muted effects on out-migration. Residents of counties whose economic conditions declined appear to display more out-group bias in response to newcomers, but not necessarily to move out. Thus, residential sorting appears to be driven by factors unrelated to economic grievance, like compositional amenities or preference for ethnic homogeneity. We show in Appendix S similar results if we divide counties by an index of economic grievance, rather than just by the relative change in the poor people.

6.4 Crime and Other Alternative Mechanisms

We lack data to test whether perceptions of threat have changed in response to the arrival of newcomers. However, we can directly test whether the threat itself has changed. In Appendix T we show that there is no evidence of changes in crime due to the arrival of migrants. In response to recent unauthorized Mexican migrants, property crimes, violent crimes, and total crimes do not increase. While we cannot evaluate the police and prosecutors' response to migrants more specifically, nor politicians' willingness to use (mis)perceptions to gain office, there is no evidence that migrants cause more crime, and that more people are being arrested or charged because of the presence of migrants. This collection of findings is evidence against direct threat explanations for electoral reactions.

6.5 Robustness for mechanisms

In Appendix U we conduct the same robustness checks on the mechanisms. The results hold. The mechanisms identified are robust to controlling for differential trends, a simulated instrument, proxies of the stock of Mexican-born and Hispanic people at the beginning of the period, removing outliers, and not using population weights. We only find statistically significant effects on pre-period values on three variables, but in all cases the sign of the coefficient is reversed, suggesting a reversal in the trend. [27]

7 Taxation and the Social Safety Net

We have documented that the inflow of recent unauthorized migrants increased the vote share of the Republican party in federal elections, reduced total public expenditure and

[27]We do not have lagged values for out-migration and the variable indicating the relative importance of universalist values.

expenditure in education, and increased relative expenditure in police and judiciary. We have shown that the effects are most likely driven by a decrease in employment in exposed sectors, like construction and hospitality, which caused a small increase in the poverty rate. Moreover, counties saw a population decline, residential sorting, and an increase in out-group bias.

In this section, we explore whether these effects vary across different policy environments. Specifically, our hypothesis is that counties with more progressive tax structures or a larger social safety net are better able to mitigate economic shocks and compensate people who lose economically. The impacts of unauthorized migration should be lower in places with progressive taxation and more robust redistribution. We find suggestive evidence that this is the case.

To capture the more progressive tax structure, we divide counties according to their ratio of revenue generated from income to sales tax in 2007, before our period of analysis. According to the Institute on Taxation and Economic Policy (ITEP), US income tax is the most progressive local tax, whereas sales tax is the most regressive (Wiehe et al., 2018). Counties with a higher share of revenues from sales tax have a more regressive tax structure. The tax measures are ideal from the perspective that tax law changes are incremental, and therefore the county-level tax measure reflects redistribution preferences from the past (Berry, 2021; Marlowe, 2014; Martell and Greenwade, 2012).

Given that this is an imperfect proxy, we explore two other, state-level, measures of progressive taxation and strength of the social safety net. First, we divide counties according to the index of fair taxation of the state in which they are located, produced by ITEP following a similar criteria as our county measure. Second, we divide states according to the share of the poor population covered by the federal program Temporary Assistance to Needy Families (TANF) in 2010, before our period of analysis (Gaines et al., 2021). This federal cash transfer program is administered at the state-level. Like Medicaid, state governments have discretionary ability to determine the operational rules (i.e. generosity of the program). The TANF to poverty ratio is a more direct measure of redistribution, and evidence shows the implementation has been devolved as well (Fording et al., 2007). These measures of regressive and progressive taxation are not well correlated with state partisanship, so we are not comparing conservative and liberal counties. Figure 16 in Appendix X shows that our state-level categories are not a good proxy for contemporary political leanings. Over the last 40 years, there has been substantial devolution of fiscal authority from states to counties and cities (Gainsborough, 2003; Xu and Warner, 2016). The county measure (described before) reflects variation because of devolution, whereas the state measure captures taxation at the level at which it is legislated.

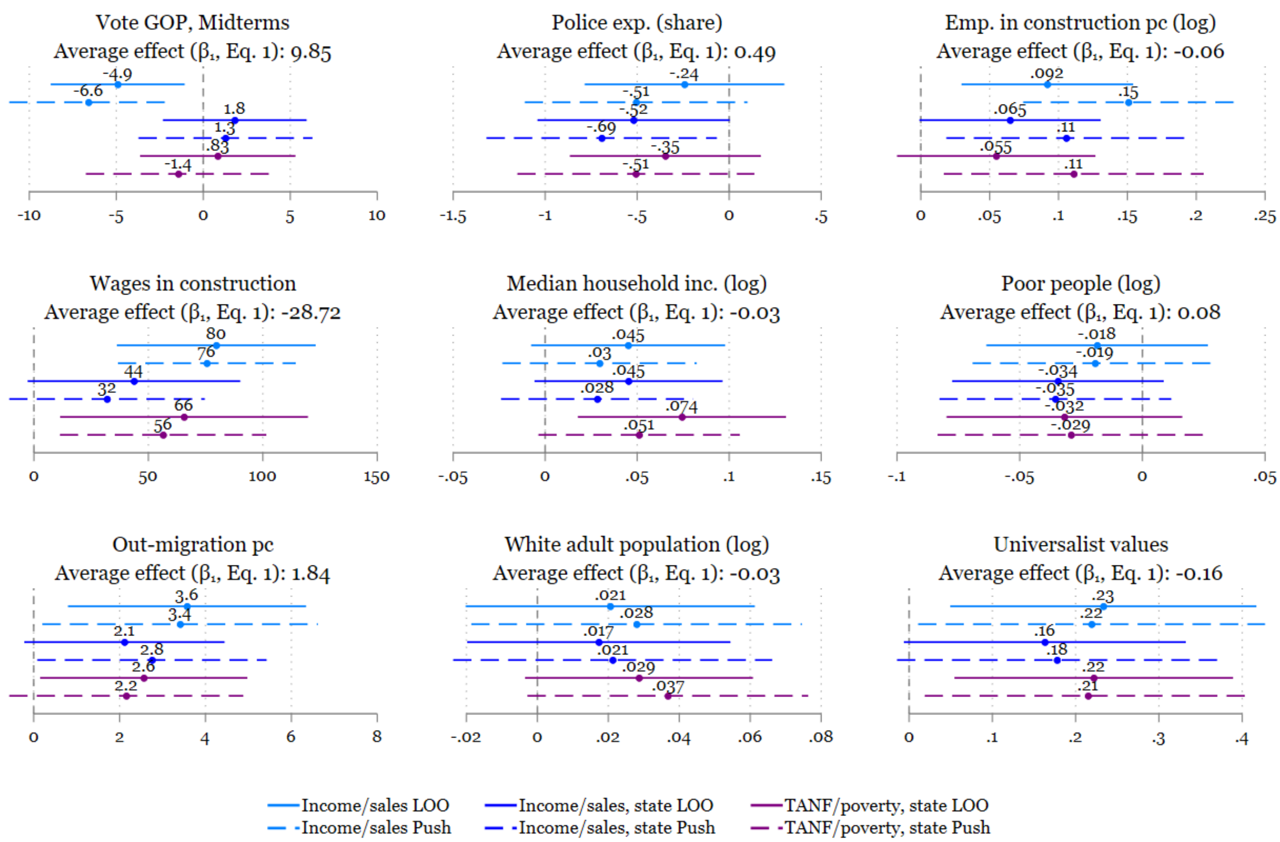

Figure 4: Heterogeneous effects by pre-period tax progressivity/strength of the safety net. Displayed are the 90% coefficient intervals of the interaction between the instrument(s) and dummy indicating above median values. Estimations are reduced form. Solid lines correspond to the LOO instrument and dashed lines to the push factors instrument.

Figure 4 presents the differential effects of unauthorized Mexican migration on some of the main outcomes and mechanisms for counties with above median values of tax progressivity/strength of the safety net. The substantive pattern that emerges is that counties with above median values have more muted shifts to the right and more modest economic, social, and moral impacts. Due to the smaller sample size, these differences are not generally statistically significant, but they are meaningful and consistent between proxies and instruments.

Our interpretation is that counties with more progressive taxation/stronger safety net appear to compensate those who lose economically, lessening the negative labor market and welfare impacts of unauthorized migration. Counties with above-median income to sales tax ratio see the boost for the GOP in midterm elections more than halved. Similarly, the formal job losses and wage declines in construction, as well as the reductions in median household income and universalism are either partially or fully reversed. Tables 28, 29, 30 in appendix X show the same information in regression format.

In line with Table 9, the counties with more progressive taxation/stronger safety net do not see more muted effects on out-migration. In fact, the opposite is true. The impact of recent migration inflows on out-migration is exacerbated. This exception supports the hypothesis that residential sorting does not respond to economic factors, but to other motivations like compositional amenities.[28]

Appendix Y explores other possible sources of heterogeneity. We test whether counties that had above median shares of adult Hispanic population, adult White population, and adult Black population in 2006, as well as above median vote shares of the GOP in the 2006 midterm election, see consistently different effects. Figure 17 plots the coefficients of the interaction terms. We do not detect any systematic difference. The extent of the reaction to recent unauthorized Mexican migrants, and the drivers of such reaction, seem to be unaffected by past political and demographic conditions.

8 Conclusion

We estimate the impact of recent unauthorized Mexican migration on the political, economic, and social conditions of US counties using two different shift-share strategies. In response to newcomer migrants, county vote share for the Republican party increased in House and presidential elections. Local government agencies reduced total expenditure, divested in education, and increased relative spending in policing and the administration of justice.

[28]The effect on adult White population is also mitigated in counties with more progressive taxation/stronger safety, suggesting that residential sorting and White flight do not exactly move together.

We contend that three interrelated mechanisms explain this political and policy response. (1) This migration creates formal job loss in the construction and hospitality and leisure sectors, "migrant-intensive" sectors. Despite creating formal job gain in manufacturing and, thus, no aggregate decline in overall employment levels, the arrival of newcomers causes an increase in the number of poor people—probably resulting from transient job loss. (2) Established residents, arguably motivated by economic grievance, display more out-group bias. (3) Newcomers generate population loss, especially among White residents, explained by out-migration. This residential sorting seems to be unrelated to economic factors.

Both our main political effects and the mechanisms are robust to conditioning on differential pre-trends, a counterfactual instrument, a proxy of the stock of migrants, and spatial lags, as well as not weighting by predicted population, removing outliers, and exploiting changes within groups of politically similar counties (as opposed to within the whole state). We do not find a statistically significant association between migration and lagged outcomes, making the parallel trends assumption more likely.

These results contribute to a growing literature on the backlash against migrants from developing countries. While responses to different groups of immigrants have been studied in the US, scholars have yet to quantitatively estimate the impacts of unauthorized migrants, arguably the most politicized of migrants. We are, to the best of our knowledge, the first ones to study specifically the political impacts of unauthorized migration throughout a country. This is theoretically relevant because unauthorized migrants have distinctive labor market and political characteristics. For example, they cannot vote, but are largely long-term residents; they are fully employed, but largely in informal sectors (New American Economy, 2021; Svajlenka, 2020).

Unlike most existing studies that focus either on the political and electoral effects or on the fiscal effects of immigration, we study and link both. The conservative reaction we document is consistent with the impacts of refugees and poorly educated migrants, especially from developing countries, in Europe and the US (Baerg et al., 2018; Barone et al., 2016; Dinas et al., 2019; Edo et al., 2019; Halla et al., 2017; Mayda et al., 2022a,b; Mendez and Cutillas, 2014; Otto and Steinhardt, 2014; Tabellini, 2020). Unlike the findings in Rozo and Vargas (2021), we cannot conclude that the response is explained by a radicalization of citizens at the extreme of the political distribution. Rather, we find little evidence of heterogeneity across the political spectrum.

We also contribute to disentangling the roles and interactions of economic and cultural factors in explaining the right-wing reaction to immigration (Alesina and Tabellini, 2021). We connect these two sets of forces with the literature on internal migration and White flights in the US (Boustan, 2010; Derenoncourt, 2022; Shertzer and Walsh, 2019; Tabellini, 2018). We provide evidence that the type of migration we study creates economic grievance, despite causing formal job loss only in certain sectors and a modest increase in the number of poor people. This grievance appears to explain an increase in out-group bias (decline in relative universalism) but does not seem to explain residential sorting. The effects we find on universal values are a novel finding in the literature on the political economy of migration, and an explicit answer to Alesina and Tabellini (2021)'s suggestion of exploring the role of migration on moral values.

We demonstrate that a county's taxation and redistribution are notable sources of heterogeneity. The social, political, and economic impacts of unauthorized migrants are concentrated in counties that have the least capacity or willingness for redistribution. Since the tax structure of a county changes slowly, it helps explain what prior party votes share does not. Places with more progressive taxation appear better able to compensate those who lose with the arrival of unauthorized migrants. These counties see formal job gains in construction. These counties also appear to have smaller impacts on the number of poor people and no impact on the decline of universalist values. Counties with regressive taxation appear to account for most of the formal job loss and associated poverty caused by unauthorized migrants and the substantive shift to the right. In counties with regressive taxation, unauthorized migrant arrivals cause more formal job loss, increase poverty, and substantially decrease support for universalist values. We argue that these counties that do not compensate economic losers are most destabilized by unauthorized migration. Ironically, in places with regressive taxation, the arrival of unauthorized migrants prompts changes in values away from universal redistribution and the kinds of policies that elsewhere mute the negative impacts of unauthorized migrants. A county's taxation and redistribution enhance, rather than hinder, out-migration, suggesting that residential sorting does not respond to economic motivations.

From the standardized coefficients, it is clear that the effects of migration on the political and policy outcomes are larger than the effect estimated with the mechanisms. The persistent conservative reaction to unauthorized Mexican newcomers cannot be fully explained by the many mechanisms we have explored. Future areas of research should document additional explanations for this shift to the right. For example, understanding attitudes toward migration at the county-level could inform these results. Furthermore, emerging work in political and behavioral economics (Ajzenman et al., 2022, 2021) and political psychology (Enos, 2017; Mutz, 2018) exploring the role of (mis)perceptions related to threat might help to explain the relative sizes of the impacts we estimate.

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Appendix A Secure Communities

We use Secure Communities, a locally implemented federal deportation program, to interrogate selection in our main explanatory variable. Later we look at the the impact of unauthorized migration on outcomes from the program. Secure Communities was a federal program that facilitated information sharing between local police and sheriff's departments and Immigration and Customs Enforcement (ICE). Local departments could submit fingerprints to ICE, which could use them to identify some individuals eligible for deportation. In turn, ICE would request that an individual be held on a detainer so that the deportation process could begin.

The program is useful to interrogate our data because it is the largest immigration program during our period of study and because it was implemented at the local level. The rollout was progressive, but not entirely random. The share of Hispanics, distance to the border, and crime rates are predictors of early adoption (Cox and Miles, 2013). We follow a collection of papers that uses the remaining exogenous variation. In our case we interrogate whether applying for a Consular ID is elastic to the policy environment, using the Secure Communities implementation. We find evidence of an inelastic decision.

Once in place, we study the program as a dependent variable and examine the intensive margin. We ask whether new flows of unauthorized migrants change how local authorities use the program. As the program became established, it was subject to political manipulation. As of 2013, local authorities were required to participate in Secure Communities. However, before it was mandatory, the program became politicized. States tried to opt out. Some counties sought to circumvent the program by refusing to submit fingerprints for individuals with no or little criminal background (Mitchell, 2011). Other counties argued that detainers from ICE were requests that could be denied and announced they would decline. County officials argued that the program was facilitating deportation of non-criminals and undermining police relations in immigrant communities (Lind, 2014; Mitchell, 2011). In 2014 the program was scaled back after federal courts held that ICE detainers were optional,[29] and counties could be held liable for due process violations of individuals detained solely at the request of ICE.[30]

[29]Galarza v. Szalczyk, 745 F.3d 634 (3d Cir. 2014)

[30]Miranda-Olivares v. Clackamas Cnty., No. 3:12-cv-02317-ST (D. Or. Apr. 11, 2014)

Appendix B Evolution of flows and stocks of unauthorized Mexican migrants

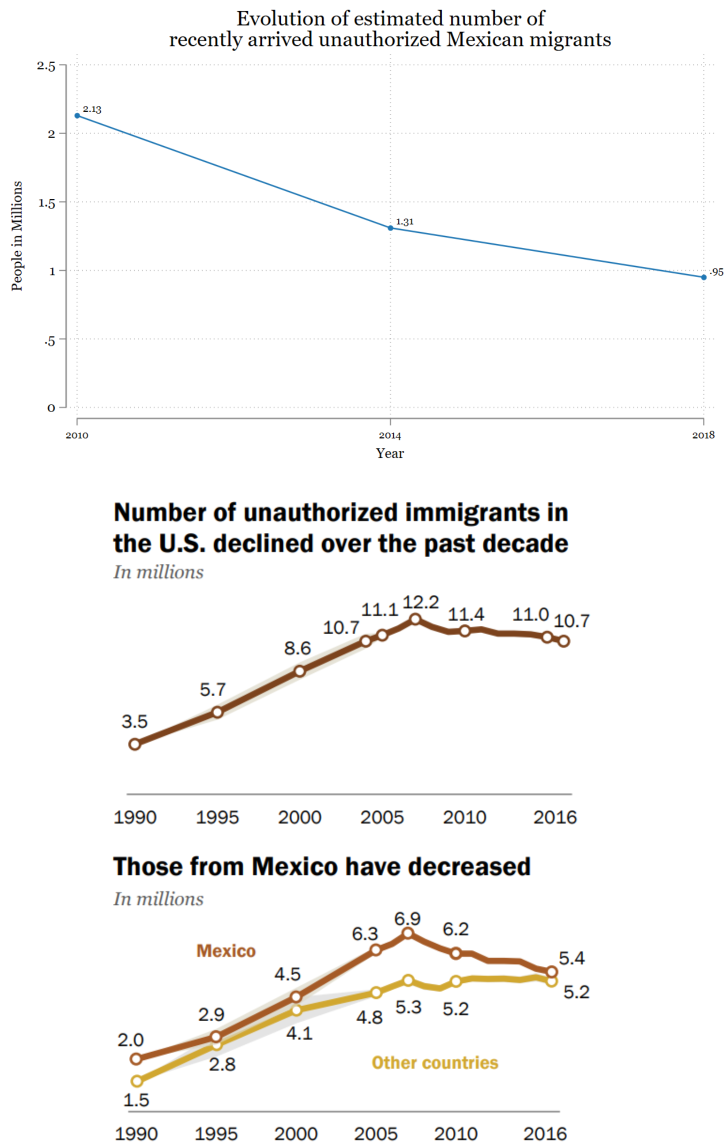

Figure 5: The top figure plots the evolution of inflows of recent unauthorized migrants. The bottom figure, created by Passel and Cohn (2018), plots the evolution of the stock of unauthorized Mexican migrants.

Appendix C Summary statistics of selected variables for newcomer Mexican unauthorized migrants using ACS 5 and Consular data

There are 441 counties in ACS-5 with detailed demographic characteristics for likely unauthorized Mexican migrants. We compare the distribution of those characteristics for those counties with that of "recently arrived migrants" in the consular data. The only substantive difference relates to age. This is not a surprising difference because children apply for consular IDs at much lower rates than adults. In our final sample, less than 2% of cardholders are under 18.

Table 10: Summary statistics of selected variables for newcomer Mexican unauthorized migrants using ACS 5 and Consular data

	(1) ACS 5	(2) Consular data same counties	(3) Consular data full sample
Female	0.41	0.41	0.41
	(0.49)	(0.49)	(0.49)
Never married/single	0.49	0.46	0.46
	(0.50)	(0.50)	(0.50)
Age	30.04	32.48	32.38
	(10.64)	(11.88)	(11.76)
Observations	45818	3677220	4380979
Number of Counties	441	441	2684

Sources: SRE, 2022 and Ruggles et al. (2022). The ACS 5 sample is comprised of people born in Mexico without US citizenship who arrived in the US less than five years before and with no college degree and between 16 and 64 years old. The Consular sample is comprised of unique new observations per period per CBSA.

Appendix D Authorized vs unauthorized migrants

Table 11: Correlation between the LOO instrument and ACS 5 estimates of unauthorized and authorized recent Mexican migration

	(1) Unauthorized	(2) Authorized
instrument	0.553***	0.028*
	(0.040)	(0.014)
Observations	974	692
F statistic	192	4
Mean of Dep. Var	0.265	0.056
Mean of Ind. Var	0.518	0.518

Sources: SRE, 2022 and Ruggles et al. (2022). The ACS 5 unauthorized sample is comprised of people born in Mexico without citizenship who arrived in the US less than five years before and with no college degree and between 16 and 64 years old. The ACS 5 authorized sample is comprised of people born in Mexico without citizenship who arrived in the US less than five years before with college degree. The Consular sample is comprised of unique new observations per period per CBSA. Standard errors clustered at the county level. Estimations control for county and state-year fixed effects and are weighted by predicted county population. Variables are proportion of county population.

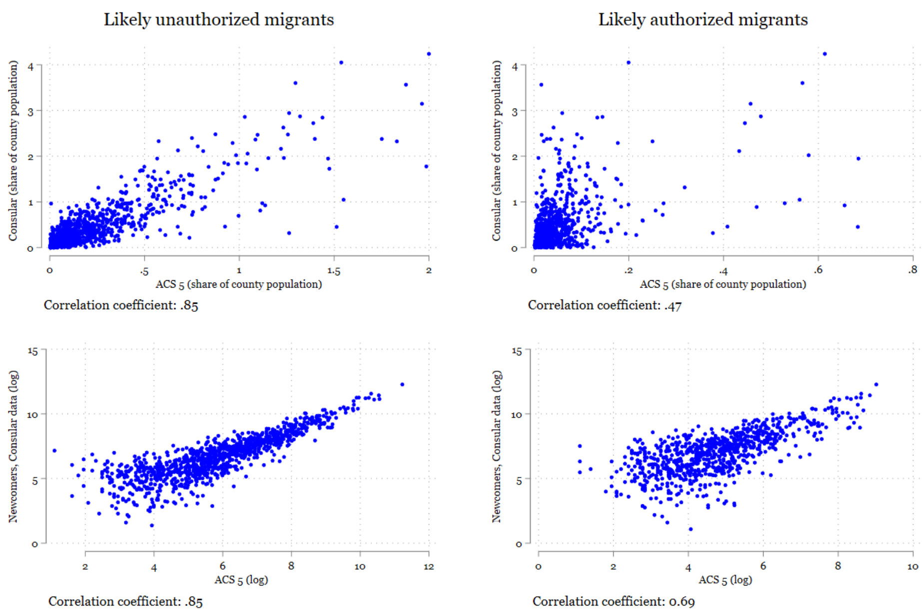

Figure 6: Comparison between estimates from Consular data and estimates of authorized and unauthorized migrants from ACS 5

Appendix E Demand for Consular IDs after drivers license changes

As of 2018, 12 states and DC allowed unauthorized migrants to get a diver's license (NCSL Immigrant Policy Project, 2021), as compared with only 3 before 2012. Using Callaway and Sant'Anna (2021)'s estimator, we implement an event-study to test whether states that modified their regulations observed an uptick in consular cards issued. Figure 7 shows the evolution of consular ID take-up by quarter from 2013 to 2016. A jump lasting three quarters, a time frame much shorter than our periods, starting before the policy went into effect, is evident. The "pre-treatment effect" is probably explained by the announcement of the program, whereas the short-lived "post-treatment effect" suggests that individuals may have waited a little longer to get their consular ID, until after the states' policies were implemented. Therefore, the evidence suggests that within our units of analysis—4 years—policy changes do not consistently alter selecting into our dataset. Furthermore, our specification controls flexibly for state by period fixed effects, so within period shocks will not bias our results

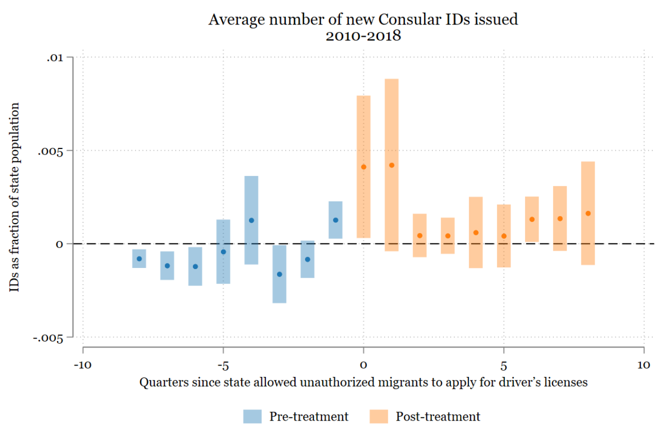

Figure 7: Effect of driver's license regulation on demand for Consular IDs

Appendix F Description of Specifications for Secure Communities

Other studies have identified the impacts of Secure Communities. East et al. (2022) identify lower employment shares among unauthorized migrants and Alsan and Yang (2019) a decline in enrollment in federal entitlement benefits like SNAP and SSI among Hispanic citizens due to fear of deportation. In theory, the activation of Secure Communities could discourage applying for a consular ID, as it would be obvious to local authorities that the cardholder is a foreign national, perhaps prompting those authorities to submit fingerprints. To identify whether applications to Consular IDs are elastic to the policy environment, we study the correlation between the activation of Secure Communities and the number of new IDs issued. Given that Secure Communities was rolled out gradually (although not randomly) we carry out six different event-study designs using Callaway and Sant'Anna (2021) generalized difference-in-differences estimator. The main differences between them are exact period of analysis and the use of controls identified in previous studies (Cox and Miles, 2013; Alsan and Yang, 2019) that correlate with the time adoption. In general, estimations progressively build to each other.

The first estimation is the simplest. Secure Communities was implemented from October 2008 to September 2013, so our period of analysis goes from the first quarter in 2006 to the fourth quarter in 2016. Always-control counties (98 out of 2678) are those that adopted the program lastly, in the first quarter of 2013. The second estimation is the same, except that it weights the regression by population. The third estimation follows Cox and Miles (2013) and controls for distance to the Mexican border and share of Hispanic population—strong correlates of time of adoption. The fourth estimation follows Alsan and Yang (2019) and, on top of controlling for distance to the Mexican border and share of Hispanic population, excludes border counties and the states of Massachusetts, New York, and Illinois. The authors argue that border counties were early adopters, possibly due to experience with immigration enforcement, and that the three mentioned states fought against the implementation of the program. The fifth estimation uses population weights on the fourth estimation. Finally, the sixth estimation uses weights and controls, like the fifth, but restricts the periods of analysis to 2008–2013. The intention is to have a larger (880) and more diverse group of always-control counties. All estimations restrict the results to eight quarters (2 years) after the activation of the program.

Figure 8 displays the evolution of take-up rates across a number of specifications reflecting different comparison options. The results, while sensitive to specifications, are consistently statistically not significant.

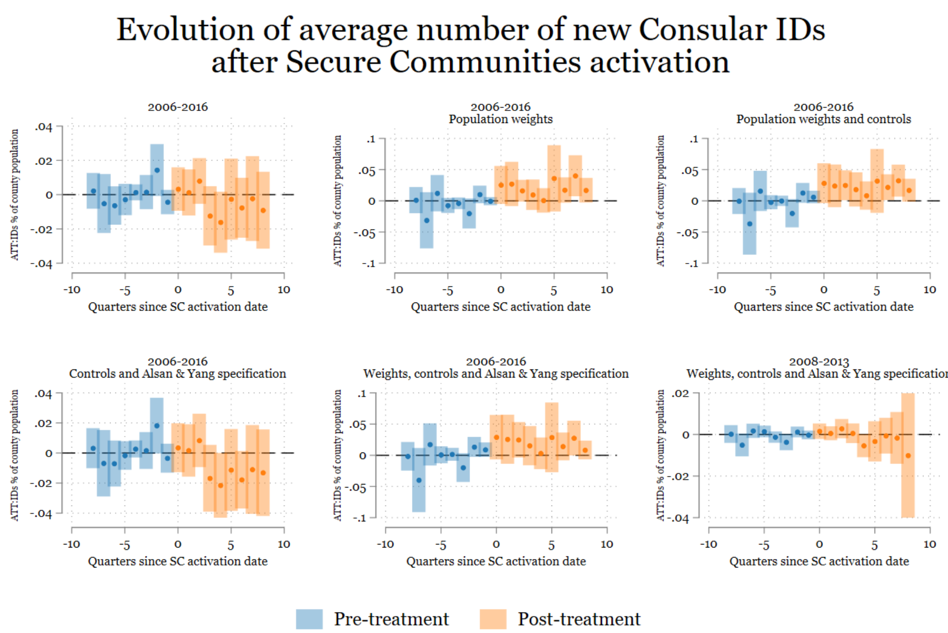

Figure 8: Secure Communities

Appendix G Correlation between observed migration and other populations

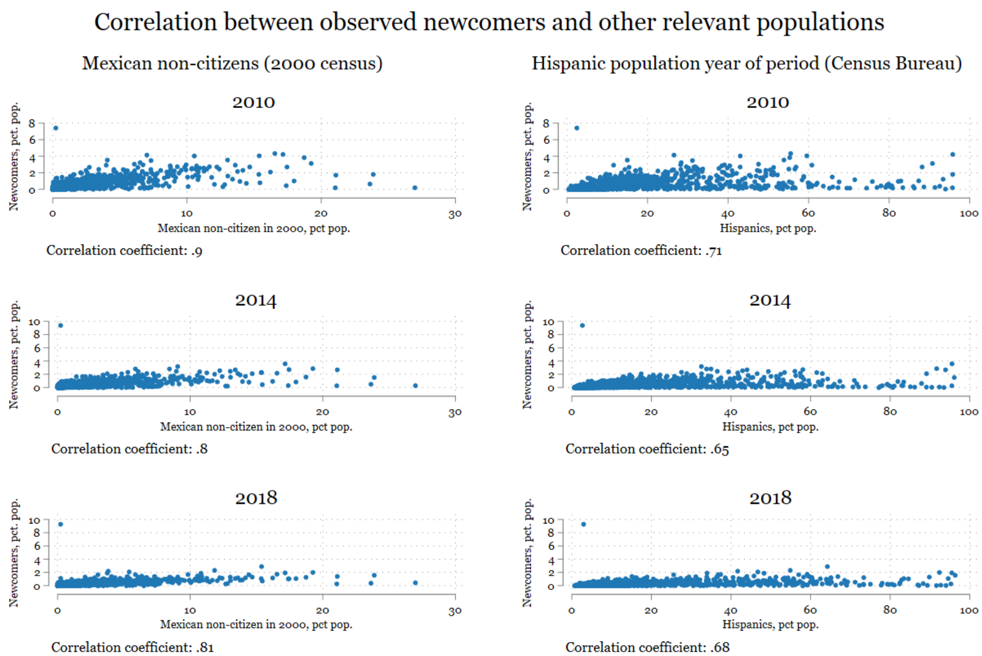

Figure 9: Correlation between observed migration and other populations

Appendix H   Summary statistics for mechansisms

Table 12: Summary statistics

	Mean	Std	Min	Max	Obs	Counties	Data relative to end of periods
Total emp, pc 15-64 log	-.67	.34	-3.97	1.71	8006	2671	1
Construction emp, pc log	-3.62	.45	-6.65	.42	7461	2579	1
Manufacturing emp, pc log	-3.07	.74	-8.03	-.33	7430	2539	1
Leisure emp, pc log	-2.75	.43	-7.26	.23	7916	2658	1
Agric emp, pc log	-6.57	1.53	-11.18	-1.14	4516	1892	1
Weekly average wages, 2010 USD	874	262	313	2401	8009	2672	1
Weekly wages, construction	993	223	227	2363	7464	2580	1
Weekly wages, manufacturing	1121	356	135	3760	7433	2540	1
Weekly wages, leisure	366	120	81	1048	7919	2659	1
Weekly wages, agric	611	198	136	1894	4516	1892	1
Real GDP, pc log	3.88	.44	2.1	8.32	7860	2629	1
Real Median HH income, log	10.88	.26	9.97	11.79	8022	2674	1
Unemployment rate	6.03	2.83	1.4	29.3	8022	2674	1
Number of poor people, log	10.89	1.63	4.04	14.4	8022	2674	1
Number of people in SNAP, log	10.81	1.65	2.77	13.99	8022	2674	1
Out-migration per 1000 people	55.24	17.06	8.63	300.25	8020	2674	1
Adult population (log)	12.63	1.59	5.66	15.85	8022	2674	1
Adult Hispanic pop (log)	10.23	2.43	2.48	15.04	8022	2674	1
Adult White pop (log)	12.36	1.52	5.59	15.49	8022	2674	1
Adult Black pop (log)	10.01	2.34	0	13.74	8013	2674	1
Relative importance univ values	.152	.497	-3.803	3.482	5802	2096	.5
All crime, pc log	-3.48	.94	-11.11	-.92	7872	2657	1.5
Violent crime, pc log	-5.87	1.03	-15.45	-3.42	7820	2652	1.5
Property crime, pc log	-3.88	.92	-11.15	-.95	7858	2656	1.5
Turnout midterms (pct)	48.23	11.6	0	136.2	7667	2674	0

Column 1 is the mean of the variable. Column 2 is the standard deviation. Column 3 is the minimum. Column 4 is the maximum. Column 5 is the total number of country-period observations. Column 6 is the number of unique counties. Column 7 reflects the year for which we have data relative to our periods: 0 indicates that the data is for the years 2010, 2014, and 2018; 1 indicates that it is for the years 2011, 2015, and 2019; 1.5 (0.5) indicates it is a 1.5 (0.5) year average after the end of our periods; -2 indicates that it is for the years 2008, 2012, and 2016. All the estimates are weighted by county population.

Appendix I Data for push factors

Our second identification strategy predicts migration from each Mexican municipality in each period. In particular, we regress the observed number of migrants on a set of time-varying variables. Then, we use the fitted values as the shifters, which we the interact with the original shares to construct the measure of predicted migration at the county level for each period. Our time-varying variables come from 4 different datasets.

1. **The University of Delaware's temperature and precipitation data**. We calculated the mean temperature and precipitation for each data point (recording station) within Mexico from 1950 until 2017. We then calculated the mean and the standard deviation of values for every period (2007-10, 2011-14 and 2015-17). To deal with municipalities with more than one data point, we took the average of all the points within one municipality. For the municipalities with no data points, we assigned them the values of the neighboring stations. Our dataset has information for 2,456 municipalities, for three periods, and the variables are mean period precipitation, mean period temperature, std period precipitation, std period temperature.

2. **The National Institute of Statistics and Geography's (INEGI) deaths data**. We used the yearly statistics of general deaths for the years 2005-2020. This dataset contains the number of deceases per municipality and the causes of such deceases. For every municipality, we calculated the number of general deaths, neonatal deaths, infant deaths, maternal deaths and homicides, both in levels and in shares of municipality population. Our dataset has mean municipality values for each of 2,493 municipalities –Mexico City's information comes at the delegation level, but we average the values in the final dataset– for each period (2007-10, 2011-14 and 2015-18).

3. **The National Institute of Statistics and Geography's (INEGI) Economic Census for 2009, 2014 and 2019**. Every five years, INEGI gathers data about the economic activity in each Mexican municipality corresponding to the previous year. Among others, the dataset has information on total investment, total production, number of employed people, wages, input expenditures and stocks for different sectors and subsectors. We constructed municipal totals for every year for the following variables: number of economic units, total production, value added, total investment, total workers, total women workers, total yearly hours worked, total employees and share of workers that are women. Moreover, for all the relevant variables, we obtained the per capita indicators. Our dataset has information for 2,465 municipalities for each period (2009, 2014 and 2019).

4. **The National Council of Social Policy Evaluation's (CONEVAL) poverty and underdevelopment estimates**. We use two datasets both covering the years 2010, 2015 and 2020. On the one hand, we use the dataset of poverty indicators. For every municipality, every year, there is data on the rates of poverty and extreme poverty, as well as indicators of underdevelopment in education, health, and housing. There is data from 2,469 municipalities for each period (2010, 2015 and 2020). On the other hand, we use the dataset of underdevelopment estimates. Among others,

it has information about the share of: adults that do not know how to read and write, children that do not go to school, households without basic health, households without concrete floors, households without toilets, households without electricity and households without washing machine. There is data from 2,469 municipalities for each period (2010, 2015 and 2020).

Once we had combined all these push factors, we created square terms to model potential non-linearities. The following table displays the summary statistics for these variables.

We then merged this dataset with the data on the observed number of migrants from each municipality in each period. Given that the variable of interest is censored at zero, we aimed to predict observed migration using a Poisson regression. To avoid over-fitting, however, we first implemented a Lasso correction. Out of the 54 variables included in the regression, 26 were selected. The following figure presents the summary of the Lasso regression as well as the coefficients selected

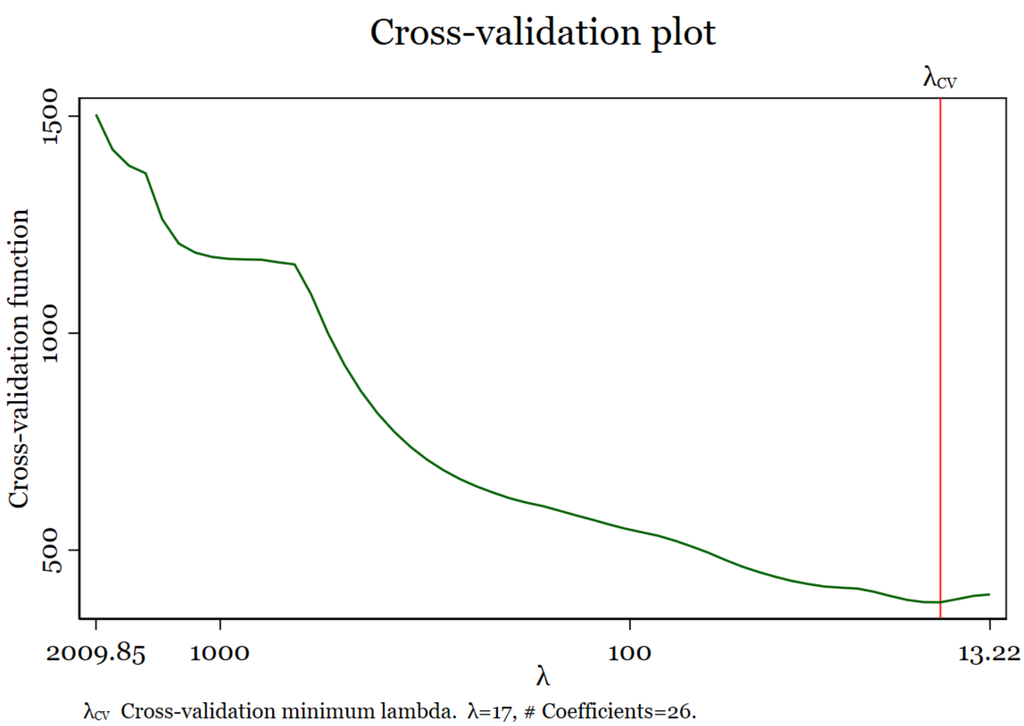

Figure 10: Poisson Lasso

With these coefficients, we estimated the Poisson regression. The predicted values correspond to the municipal emigrants for each period. The next table presents the results of the Poisson regression.

Table 13: Summary statistics of push factors

	Mean	Std	Min	Max	Obs
Share adults that cannot read	11.98	8.76	.73	66.55	7326
Share of kids no school	5.21	3.26	0	42.3	7326
Share of HH, no access to health	25.54	15.84	.88	98.14	7326
Share of HH, no concrete floors	9.83	10.21	0	79.37	7326
Share of HH, no toilets	6.19	7.9	0	91.81	7326
Share of HH, no electricity	2.98	4.56	0	68.69	7326
Share of HH, no washing machine	49.06	24.85	4.56	100	7326
Share of HH, no wash machine, sq	3024.56	2729.2	20.76	10000	7326
Social underdevelopment	.01	1	-1.85	6.83	7326
Poverty rate	65.06	21.15	2.73	99.94	7323
Extreme poverty rate	20.93	18.42	0	97.46	7323
Education underdevelopment	28.44	10.43	2.63	65.38	7323
Health underdevelopment	24.65	15.9	.93	98.23	7323
Housing underdevelopment	44.66	30.19	.04	100	7323
Mean precipitation	81.83	51.59	6.34	348.14	7323
Std precipitation	10.45	6.78	.01	43.41	7323
Deviation from historic precip	-11.11	10.21	-66.43	84.01	7323
Mean temperature	20.248	4.13	9.85	29.867	7323
Std temperature	.29	.18	0	1.09	7323
Deviation from historic temp	.27	.39	-1.43	2.16	7323
Homicides	9.54	58.45	0	1998.25	7420
Homicide rate	18.05	31.08	0	840.34	7328
Neo-natal deaths	10.61	80.62	0	3977.41	7420
Neo-natal death rate	10.33	22.88	0	929.87	7328
Maternal deaths	.3	2.32	0	117.31	7420
Maternal death rate	.34	1.53	0	85.03	7328
Infant death rate	16.35	31.46	0	1298.36	7328
Economic units	1811.6	12823.49	2	613963.1	7331
Economic units, per capita	.03	.03	0	.4	7323
Total production	6463.16	72209.38	-558.43	4287579	7327
Total production, per capita	.04	.27	-.01	11.24	7319
Value added	2891.44	38738.59	-12455.91	2327419	7327
Value added, per capita	0	.13	-.14	7.33	7319
Total investment	225.29	3137.17	-16160.32	192598.2	7327
Total investment, per capita	0	.01	-.04	.39	7319
Total workers	9598.1	92629.86	5	4836362	7327
Total workers per capita	.1	.09	0	1.43	7319
Total women workers	3916.02	37643.61	2	1968612	7327
Total women workers, per capita	.04	.04	0	.81	7319
Yearly hours worked	22753.07	222108.6	8.71	1.18e+07	7327
Yearly hours worked, per capita	.21	.2	0	2.82	7319
Total employees	5515.56	56280.58	0	2964527	7327
Total employees, per capita	.04	.06	0	1.41	7319
Share of women workers	47.73	10.94	6.43	98.65	7331
Population	51109.17	305502.4	93	1.38e+07	7328
Log population	9.45	1.56	4.53	16.44	7328
Population, square	9.59e+10	3.80e+12	8649	1.92e+14	7328
Homicide rate, square	1291.42	11645.62	0	706164.8	7328
Poverty rate, square	4680.37	2638.17	7.48	9987.65	7323
Extreme poverty rate, square	777.46	1252.2	0	9498.46	7323
Social underdevelop, square	.99	1.77	0	46.61	7326
Share adults that cannot read sq	220.32	332.33	.53	4428.55	7326
Total production, square	5.26e+09	2.45e+11	0	1.84e+13	7327
Mean temperature, square	427.06	167.66	97.02	892.02	7323
Share of kids no school, square	37.75	64.31	0	1789.58	7326

Table 14: Lasso Coefficients

	(1) Recently arrived Mexican migrants
Share adults that cannot read	0.000269
Share of HH, no access to health	0.00736
Share of HH, no concrete floors	0.0107
Share of HH, no toilets	0.0151
Education underdevelopment	0.0394
Deviation from historic precip	0.00258
Deviation from historic temp	-0.353
Mean precipitation	-0.00438
Mean temperature	0.00753
Std precipitation	-0.00664
Std temperature	0.350
Homicides	0.000170
Neo-natal deaths	0.00108
Maternal deaths	0.000877
Population	-0.000000220
Homicide rate	0.00264
Neo-natal death rate	0.00493
Total investment	-0.00000299
Economic units, per capita	1.244
Total production, per capita	-0.107
Log population	0.724
Social underdevelop, square	-0.0523
Total production, square	5.65e-14
Neo-natal death rate, square	-0.00000383
Share of HH, no washing machine, square	-0.000115
Share of kids no school, square	-0.00108
Constant	-2.092
Observations	7276

Table 15: Poisson regression

	(1)
	Recently arrived Mexican migrants
Education underdevelopment	0.041***
	(0.000)
Deviation from historic precip	0.010***
	(0.000)
Deviation from historic temp	-0.311***
	(0.001)
Economic units, per capita	1.794***
	(0.028)
Homicides	0.000***
	(0.000)
Homicide rate	0.004***
	(0.000)
Log population	0.768***
	(0.001)
Maternal deaths	-0.016***
	(0.000)
Mean precipitation	-0.005***
	(0.000)
Mean temperature	0.009***
	(0.000)
Neo-natal deaths	0.002***
	(0.000)
Neo-natal death rate	0.019***
	(0.000)
Neo-natal death rate, square	-0.000***
	(0.000)
Share of HH, no access to health	0.009***
	(0.000)
Population	-0.000***
	(0.000)
Share adults that cannot read	0.040***
	(0.000)
Share of kids no school, square	-0.004***
	(0.000)
Social underdevelop, square	-0.114***
	(0.001)
Std precipitation	-0.013***
	(0.000)
Std temperature	0.478***
	(0.003)
Total investment	-0.000***
	(0.000)
Total production, per capita	-0.415***
	(0.004)
Total production, square	0.000***
	(0.000)
Share of HH, no toilets	0.014***
	(0.000)
Share of HH, no washing machine, square	-0.000***
	(0.000)
Share of HH, no concrete floors	0.028***
	(0.000)
r2_p	0.80
N	7276

Appendix J Spatial auto-correlation in number of observed migrants

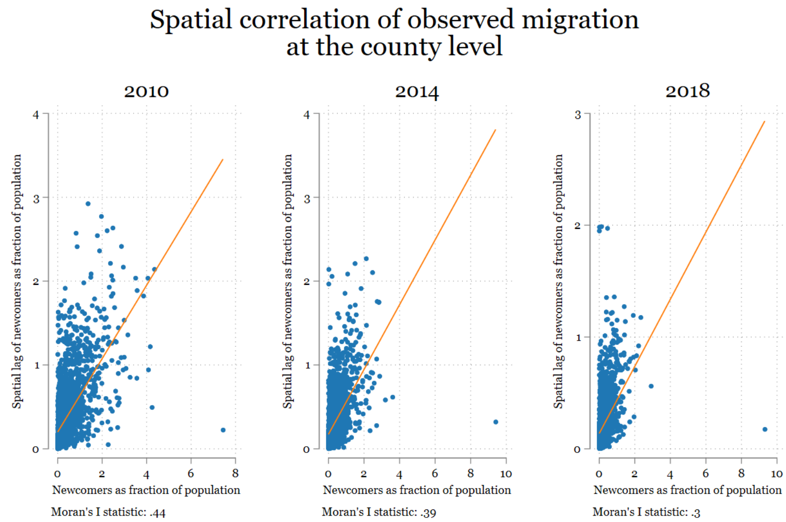

Figure 11: Spatial auto-correlation

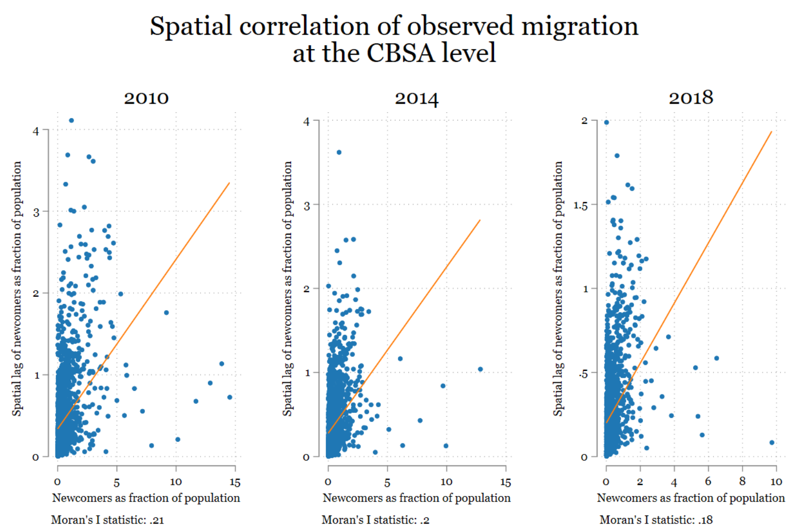

Figure 12: Spatial auto-correlation

Appendix K Correlation between instrument and stock of Mexican-born population

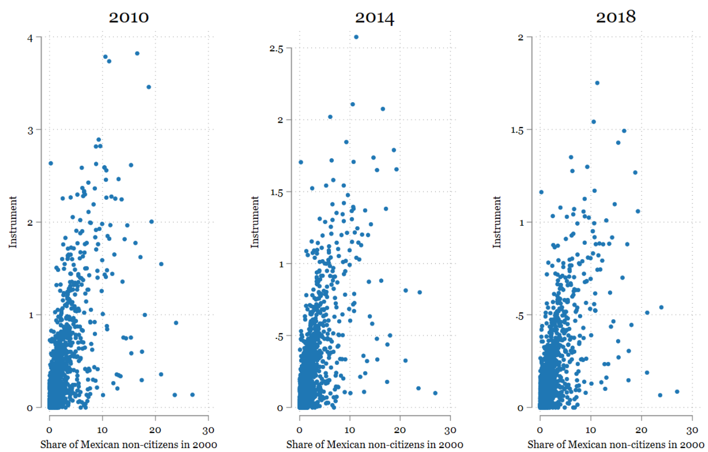

Figure 13: Correlation between instrument and estimated share of Mexican non-citizens in 2000

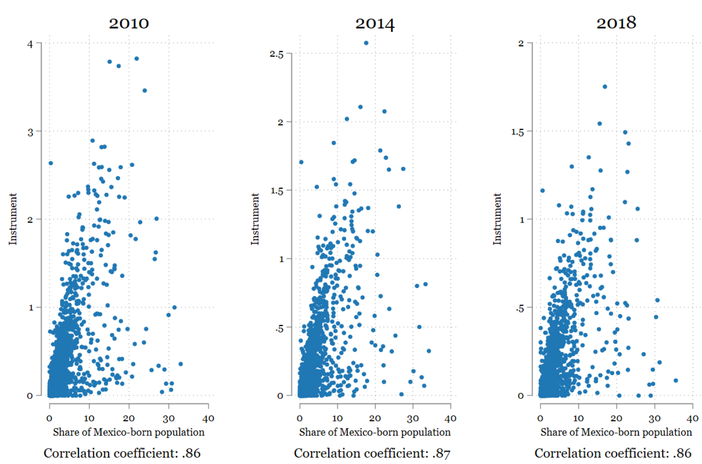

Figure 14: Correlation between instrument and estimated share of Mexico-born people at the beginning of period

Appendix L Rotemberg weights

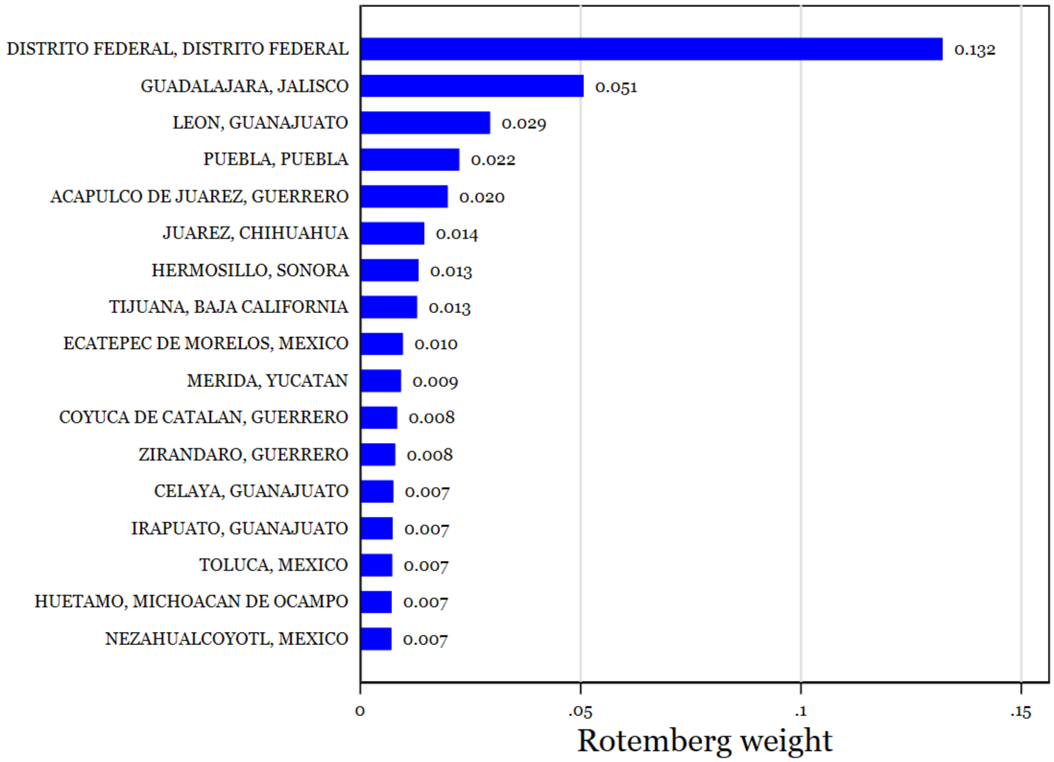

Figure 15: Rotemberg weight for each municipality using the push factors. Made with code from Goldsmith-Pinkham et al. (2020)

Appendix M Suggestive evidence of migrants' selection into economically promising areas

Table 16: Association between economic prosperity and observed migration

	Newcomers, percent population	Newcomers, percent population
Real GDP per capita (log)	0.259***	
	(0.071)	
Yearly real GDP growth		0.001**
		(0.000)

Dependent variable is observed migration as share of county population. Explanatory variables are measured the year before the beginning of the periods: 2006, 2010 and 2014. Sources: US Census; ACS-5; USDA's Economic Research Service and LAUS. Standard errors clustered at CBSA level. Estimations control for county and state-period FE. Estimations weighted by predicted population. Stars indicate *p<0.1,**p<0.05,***p<0.01

Appendix N Disaggregated effect on revenue

Table 17: Effect of arrival of unauthorized Mexican migrants on local revenues (2012 and 2017)

		Revenue categories (log pc 2010 USD)					Share of Total Revenue				
	(1) Total	(2) Own source	(3) Total Tax	(4) Property Tax	(5) Income Tax	(6) Intergov. rev.	(7) Own source	(8) Total Tax	(9) Property Tax	(10) Income Tax	(11) Intergov. rev.
A. OLS											
Newcomers, pct. pop.	-0.02*	-0.08***	-0.04***	-0.01	-2.10***	-0.02	-4.25***	-0.80**	0.08	-0.17	0.22
	(0.01)	(0.03)	(0.01)	(0.02)	(0.55)	(0.02)	(1.54)	(0.39)	(0.39)	(0.13)	(0.73)
B. 2SLS Loo											
Newcomers, pct. pop.	-0.03**	-0.04	-0.03*	0.01	-2.03***	-0.03	-1.63	-0.59	0.55	-0.16	-0.27
	(0.01)	(0.03)	(0.02)	(0.02)	(0.54)	(0.03)	(1.66)	(0.44)	(0.43)	(0.13)	(0.76)
Std. Coefficient	-0.05	-0.05	-0.04	0.02	-0.23	-0.05	-0.09	-0.03	0.03	-0.03	-0.02
$\hat{\beta} * \bar{x}$	-0.02	-0.02	-0.02	0.01	-0.36	-0.02	-0.90	-0.32	0.30	-0.09	-0.15
C. 2SLS Push Factors											
Newcomers, pct. pop.	-0.03**	-0.07**	-0.06***	-0.02	-2.17***	-0.01	-3.51*	-1.23***	-0.01	-0.17	0.48
	(0.01)	(0.03)	(0.01)	(0.02)	(0.52)	(0.03)	(1.82)	(0.43)	(0.46)	(0.13)	(0.74)
Std. Coefficient	-0.05	-0.09	-0.08	-0.03	-0.25	-0.02	-0.19	-0.07	-0.00	-0.03	0.03
$\hat{\beta} * \bar{x}$	-0.01	-0.04	-0.03	-0.01	-0.38	-0.01	-1.94	-0.68	-0.00	-0.09	0.27
Observations	5338	5338	5338	5338	896	5338	5338	5338	5338	5338	5338
Dep. Var., Mean	1.57	1.03	0.50	0.19	-1.60	0.50	59.46	36.31	27.58	1.31	36.11
Dep. Var., Sd	0.38	0.47	0.49	0.54	1.63	0.45	12.44	11.95	12.18	3.69	11.15
Ind. Var., Mean	0.55	0.55	0.55	0.55	0.18	0.55	0.55	0.55	0.55	0.55	0.55
Ind. Var., Sd	0.68	0.68	0.68	0.68	0.23	0.68	0.68	0.68	0.68	0.68	0.68
Inst. Loo, Mean	0.49	0.49	0.49	0.49	0.12	0.49	0.49	0.49	0.49	0.49	0.49
Inst. Loo, Sd	0.63	0.63	0.63	0.63	0.19	0.63	0.63	0.63	0.63	0.63	0.63

Dependent variables in columns 1-6 are in log 2010 dollars per capita. Dependent variables in columns 7-11 are shares of total revenue. Sources: Annual Survey of State and Local Government Finances. Newcomers are the new consular IDs per county per 4-year period as a proportion of predicted population. All regressions control for county and state-period fixed effects, and are weighted by predicted population. Standard errors are clustered at the CBSA level. Stars indicate *p<0.1,**p<0.05,***p<0.01

Appendix O Short vs long term effects

Table 18: Long vs short-term effects

	Midterms	Pres year		Log pc		Share of Expend	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	House	House	Pres	D. Exp	Educ	Police	Judicial
A. Reduced form, baseline without period 1							
Instrument	7.42**	-3.47	-0.47	0.24***	0.06***	0.01	-0.26***
	(3.62)	(4.16)	(1.77)	(0.05)	(0.02)	(0.20)	(0.10)
B. Short vs long-term							
Instrument	-0.90	-30.64***	-15.65***	0.32	-0.02	-2.29*	-1.87***
	(9.40)	(7.78)	(5.83)	(0.32)	(0.12)	(1.30)	(0.54)
Lagged instrument	4.35	14.21***	7.94***	-0.05	0.05	1.43*	1.00***
	(4.51)	(4.05)	(2.70)	(0.20)	(0.07)	(0.80)	(0.33)

Dependent variables in columns 1-3 are the vote share for Republicans in different federal elections. Dependent variables in column 4-5 are the log of per capita (per child population in column 4) expenditure. Columns 6-7 are the share of direct expenditure. All estimations are reduced form. Sources: Dave Leip's US Election Data; Annual Survey of State and Local Government Finances; US Census; ACS-5; USDA's Economic Research Service; Peter K. Schott's Data; County Business Patterns; Acemoglu et al. (2016). and QCEW. Sources: Dave Leip's US Election Data, Annual Survey of State and Local Government Finances, US Census, ACS-5, USDA's Economic Research Service, and QCEW. Panel one is the baseline estimation without period 1. Panel 2 implements the Jaegger et al. (2018) correction to identify short term vs long term effects. Standard errors clustered at CBSA level, except for columns 4-7 (robust standard errors). Estimations control for county and state-period FE, except for columns 4-7 (only state fixed effects). Estimations weighted by predicted population. Stars indicate *p<0.1,**p<0.05,***p<0.01

Appendix P Alternative standard errors

Adão et al. (2019) show that in shift-share designs, standard errors are correlated with initial share composition. They argue that accounting for such association is more accurate than using heteroskedastic standard errors or geographically clustered standard errors, as we do.

Both Stata and R have commands to implement their proposed correction. However, they cannot easily accommodate a large set of fixed effects. Therefore, we implement a correction inspired by them, but easier to implement. The basis of such correction is cluster analysis. We use different techniques to group counties based on the values of their 2,439 initial shares. We vary the number of clusters (from 200 to 1000) and the technique to construct them: we use both kmeans and hierarchical clustering (single). Finally, we cluster the standard errors at the level of such groups. The main problem with this approach is that we have several groups with one county (which is similar to our main estimation, where we cluster the standard errors at the CBSA level) and one group with close to a thousand counties. To provide different, more balanced groups, we obtain, via principal components analysis, the 10 first components of the 2,439 shares. We use these components in two ways. On the one hand, we carry out cluster analysis in those factors only. On the other hand, we create another group based only on the values of the first component. We do not use cluster analysis in this case, but rather divide the sample into 500 equally sized groups. This approach forms more intuitive groups. For example, the last one of them is composed of Los Angeles County, Cook County (Chicago), Orange County, Harris County (Houston), and Maricopa County (Phoenix).

Tables 19 presents the 2SLS results of these approaches with the main results. None of them consistently changes the significance.

Table 19: Alternative standard error calculation

	Midterms	Pres year		Log pc		Share of Expend	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	House	House	Pres	D. Exp	Educ	Police	Judicial
A. Baseline							
Instrument	9.850***	4.043***	5.573***	-0.049***	-0.059***	0.493***	0.311***
	(1.215)	(1.372)	(0.768)	(0.018)	(0.021)	(0.165)	(0.117)
Clustered at state-level							
Instrument	9.856***	4.046**	5.581***	-0.049***	-0.059***	0.494**	0.313**
	(1.423)	(2.011)	(1.334)	(0.016)	(0.017)	(0.192)	(0.134)
Eicker Huber White							
Instrument	9.850***	4.043***	5.573***	-0.049***	-0.059***	0.493***	0.311**
	(1.199)	(1.395)	(0.688)	(0.018)	(0.019)	(0.159)	(0.126)
PCA 1							
Instrument	9.833***	4.018***	5.549***	-0.050***	-0.059***	0.491***	0.312**
	(1.252)	(1.317)	(0.703)	(0.015)	(0.021)	(0.130)	(0.129)
Kmeans, 200 (pca)							
Instrument	9.833***	4.018*	5.549***	-0.050***	-0.059***	0.491***	0.312**
	(2.021)	(2.386)	(1.868)	(0.018)	(0.019)	(0.159)	(0.133)
Kmeans, 400 (pca)							
Instrument	9.833***	4.018**	5.549***	-0.050***	-0.059***	0.491***	0.312**
	(1.719)	(2.023)	(1.484)	(0.019)	(0.019)	(0.167)	(0.129)
Kmeans, 600 (pca)							
Instrument	9.833***	4.018**	5.549***	-0.050***	-0.059***	0.491***	0.312**
	(1.588)	(1.838)	(1.323)	(0.018)	(0.019)	(0.160)	(0.127)
Kmeans, 800 (pca)							
Instrument	9.833***	4.018**	5.549***	-0.050***	-0.059***	0.491***	0.312**
	(1.499)	(1.731)	(1.221)	(0.018)	(0.019)	(0.158)	(0.127)
Kmeans, 1000 (pca)							
Instrument	9.833***	4.018**	5.549***	-0.050***	-0.059***	0.491***	0.312**
	(1.444)	(1.651)	(1.123)	(0.018)	(0.019)	(0.161)	(0.127)
Kmeans, 1000 (all shares)							
Instrument	9.833***	4.018**	5.549***	-0.050***	-0.059***	0.491***	0.312**
	(1.579)	(1.730)	(1.293)	(0.018)	(0.019)	(0.161)	(0.126)
Hierarchical, 800 (all shares)							
Instrument	9.833***	4.018	5.549***	-0.050***	-0.059***	0.491***	0.312**
	(2.179)	(2.485)	(1.922)	(0.018)	(0.020)	(0.153)	(0.136)
Kmeans, 800 (all shares)							
Instrument	9.833***	4.018**	5.549***	-0.050***	-0.059***	0.491***	0.312**
	(1.723)	(1.950)	(1.491)	(0.018)	(0.019)	(0.156)	(0.129)

Row 1 is the baseline 2SLS specification. Row 3 clusters the standard errors (SE) at the state level. Row 3 uses Eicker Huber White SE. Row 4 clusters the SE by the distribution of the first component of all 2,439 shares —obtained after carrying out a principal component analysis. Counties are assigned to one of 500 groups. Rows 5-9 clusters SE at the level of one of 200-1000 groups obtained by classifying counties according to their first 10 components using kmeans. Rows 10-11 clusters SEs at the level of one of 800-1000 groups obtained by classifying counties according to their shares using kmeans. Row 12 clusters SE at the level of 800-1000 groups obtained by classifying counties according to their shares using hierarchical clusters (single linkage). Estimations control for county and state-period fixed effects. Estimations weighted by predicted population. Stars indicate *p<0.1,**p<0.05,***p<0.01

Appendix Q Robustness checks with push factor instrument

Table 20: Robustness checks

	Midterms	Pres year		Log pc		Share of Expend	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	House	House	Pres	D. Exp	Educ	Police	Judicial
A. Reduced form, baseline							
Instrument	10.40***	4.40***	6.81***	-0.05***	-0.04**	0.48**	0.27**
	(1.47)	(1.70)	(0.93)	(0.02)	(0.02)	(0.20)	(0.12)
B. Lagged outcome (LO)							
Instrument	1.22	5.06**	4.56***	0.00	-0.04	-0.07	0.09
	(2.40)	(2.35)	(0.78)	(0.02)	(0.02)	(0.18)	(0.10)
C. Mex non-citizen, sh							
Instrument	9.46***	8.85***	10.75***	-0.05**	-0.04	0.38*	0.24
	(1.70)	(2.00)	(1.14)	(0.02)	(0.02)	(0.21)	(0.15)
D. Hispanics, sh							
Instrument	10.77***	5.37***	8.50***	-0.05**	-0.04**	0.47**	0.24**
	(1.69)	(1.85)	(0.90)	(0.02)	(0.02)	(0.21)	(0.12)
E. Adult HS completion							
Instrument	12.15***	6.45***	8.81***	-0.05***	-0.05**	0.49**	0.27**
	(1.45)	(1.62)	(0.68)	(0.02)	(0.02)	(0.20)	(0.12)
F. China shock							
Instrument	8.35***	2.48	4.38***	-0.05***	-0.05***	0.55***	0.30**
	(1.65)	(1.75)	(0.92)	(0.02)	(0.02)	(0.19)	(0.12)
G. Simulated instrument							
Instrument	7.98***	10.68**	12.54***	-0.07*	-0.10***	0.34	0.40***
	(2.98)	(4.15)	(1.97)	(0.04)	(0.04)	(0.29)	(0.15)
H. Spatial lag							
Instrument	7.72***	4.75*	5.39***	-0.05**	-0.02	0.39*	0.18
	(1.66)	(2.49)	(1.23)	(0.02)	(0.03)	(0.20)	(0.14)
I. Stock Mex foreign							
Instrument	10.80***	5.48***	7.08***	-0.05***	-0.05**	0.52***	0.28**
	(1.45)	(1.65)	(0.94)	(0.02)	(0.02)	(0.18)	(0.11)
J. Stock Hispanics							
Instrument	9.55***	3.10*	6.43***	-0.05**	-0.04*	0.51**	0.26*
	(1.34)	(1.62)	(0.90)	(0.02)	(0.02)	(0.23)	(0.13)
K. No-outliers							
Instrument	11.67***	4.42	7.73***	-0.06**	-0.03	0.67***	0.24
	(1.45)	(2.96)	(1.35)	(0.03)	(0.03)	(0.24)	(0.18)
L. No pop weights							
Instrument	11.63***	6.70***	8.40***	-0.04	-0.06**	0.85***	0.37***
	(1.70)	(1.33)	(0.95)	(0.03)	(0.03)	(0.29)	(0.10)
*M. County-group * period FE*							
Instrument	10.66***	7.86***	7.41***	-0.10**	-0.09**	0.79**	0.35**
	(2.00)	(1.48)	(1.05)	(0.04)	(0.04)	(0.39)	(0.15)

Dependent variables in columns 1–3 are the vote share for Republicans in different federal elections. Dependent variables in column 4–5 are the log of per capita (per child population in column 4) expenditure. Columns 6–7 are the share of direct expenditure. All estimations are reduced form. Sources: Dave Leip's US Election Data; Annual Survey of State and Local Government Finances; US Census; USDA's Economic Research Service; Peter K. Schott's Data; County Business Patterns; ACS 5 from the Social Explorer; Acemoglu et al. (2016). and QCEW. Panel 1 is reduced form estimation. Panel 2 estimates effect on lagged dependent variables. Panel 3 controls for pre-2007 features interacted with period dummies. Panel 4 includes simulated instrument following Borusyak and Hull (2020). Panel 5 controls for the spatial lag of the instrument (values of neighboring counties). Panel 6 controls for the stock of Mexican non-citizens and Hispanics at the beginning of period. Panel 7 excludes outliers, does not use predicted population weights, and instead of state-period fixed effects uses politically similar counties-period fixed effects. Standard errors clustered at CBSA level. Estimations control for county and state-period fixed effects, except for row M. Estimations weighted by predicted population, except row L. Stars indicate *p<0.1,**p<0.05,***p<0.01

Appendix R Effects for ideology

Table 21: Ideology effects of arrival of unauthorized Mexican migrants (2010-19)

	Partisan Identity	Ideology	
	(1) Republican	(2) Conservative	(3) Conservative or very conservative
A. OLS			
Newcomers, pct. pop.	0.29	0.15	0.00
	(0.26)	(0.19)	(0.00)
B. 2SLS Loo			
Newcomers, pct. pop.	0.12	0.12	0.01**
	(0.30)	(0.22)	(0.00)
Std. Coefficient	0.02	0.03	0.07
$\hat{\beta} * \bar{x}$	0.05	0.06	0.00
C. 2SLS push factor			
Newcomers, pct. pop.	0.06	0.07	0.00
	(0.29)	(0.22)	(0.00)
Std. Coefficient	0.01	0.02	0.04
$\hat{\beta} * \bar{x}$	0.03	0.03	0.00
Observations	5223	5113	1883
Dep. Var., Mean	6.32	5.17	0.35
Dep. Var., Sd	3.12	2.23	0.06
Ind. Var., Mean	0.47	0.47	0.52
Ind. Var., Sd	0.59	0.59	0.61
Inst. Loo, Mean	0.41	0.42	0.48
Inst. Loo, Sd	0.56	0.56	0.59

Dependent variable in Column 1 is partisan identity, ranging from 1 (Strong Democrat) to 7 (Strong Republican). Dependent variable in Column 2 is ideology, ranges from 1 (Very Liberal) to 5 (Very Conservative). These two variables come from the Cooperative Election Study. Dependent variable in Column 3 is the share of the Metropolitan Statistical Area (MSA) that identifies as Conservative or Very Conservative, obtained from the Gallup Daily Poll —all the counties within one MSA take the same value. Sources: Cooperative Election Study; Gallup Daily Poll; US Census Bureau: Population Division and Small Area. Newcomers are the number of new consular IDs per county per 4-year period as a proportion of predicted population as described in Section 3. Source: SRE. Instrument is as described in Section 3. Standard errors clustered at the CBSA level. All estimations control for county and state-period fixed effects. Estimations weighted by predicted population. Stars indicate *p<0.1,**p<0.05,***p<0.01

The Cooperative Congressional Election Study (CCES) currently known as the Cooperative Election Study (CES) is a survey of political ideas and behaviors. The cumulative data-set contains 557,455 observations across 3,079 counties. We use an ideology measure that quantifies political leanings in five categories from very liberal to very conservative. For partisan identity, we use a seven-category measure that ranges from strong Democrat to strong Republican. We adjust using the weights provided by CES, and exclude counties with less than five observations per year, dropping all the "Not Sure" and "Don't Know" responses.

The Gallup Daily Tracker was a poll carried out by Gallup between 2008 and 2018 daily to 1,000 people around the country. One of the questions asked is "How would you describe your political views?", with categories going from Very Conservative to Very Liberal. The most detailed version is at the ZIP code level but, as of now, we only have access to the MSA level. Moreover, we only have data for 2008-2016, so we use the values for 2008, 2012, and 2016 for the analysis.

Appendix S Effects on values and out-migration by economic grievance

Table 22: Effects of arrival of unauthorized Mexican migrants on values and out-migration (2010-19), by index of economic grievance

	(1) Universalist values	(2) Out migration
A. OLS		
Newcomers, pct. pop.	-0.00	1.28*
	(0.04)	(0.72)
Above-median	-0.30***	-1.45
index × Newcomers, pct. pop.	(0.10)	(1.43)
B. Reduced form Loo		
Newcomers, pct. pop.	-0.06	2.34***
	(0.06)	(0.88)
Above-median	-0.34**	-3.82*
index × Newcomers, pct. pop.	(0.15)	(2.12)
C. Reduced form push factor		
Newcomers, pct. pop.	-0.10	2.17**
	(0.07)	(0.95)
Above-median	-0.46***	-3.28
index × Newcomers, pct. pop.	(0.17)	(2.39)
Observations	5400	7149
Dep. Var., Mean	0.20	53.48
Dep. Var., Sd	0.46	15.42
Dep. Var. above, Mean	0.06	57.57
Dep. Var. above, Sd	0.54	18.43
Inst. Loo, Mean	0.51	0.51
Inst. Loo, Sd	0.60	0.60
Inst. Loo. above, Mean	0.24	0.24
Inst. Loo. above, Sd	0.40	0.40

The dependent variable in column 1 is the average relative importance of universalist values, taken from Enke (2020). The dependent variable in column 2 is out-migration, calculated as the number of out-migrants (in thousands) divided by county population. Panels B and C are reduced form. We interact the corresponding instruments and fixed effects with an indicator of whether the county had an above-median value of an index of economic grievance. To calculate the index, we first calculate the relative change in the number of poor people, employment in construction per capita, and employment in hospitality and leisure per capita during the three periods (2011–2015–2019). Then, we subtract the relative change in construction and hospitality and leisure from the relative change in the number of poor people —conceptually, a high relative change in poor people breeds economic grievance but a high change in employment does the opposite. Sources: Enke, (2020); US Census Bureau: Population Division and Small Area; US Census Bureau: 2007-2011, 2011-2015, and 2015-2019 American Community Surveys 5; QCEW; SAIPE. Regressions are reduced form. Newcomers are the number of new consular IDs per county per 4-year period as a proportion of predicted population as described in Section 3. Source: SRE. Instrument is as described in Section 3. Standard errors clustered at the CBSA level. All estimations control for county and state-period fixed effects. Estimations weighted by predicted population. Stars indicate *p<0.1,**p<0.05,***p<0.01

Appendix T Effects on crime

The perception of immigrants as a criminal threat is widely theorized. Studying crime interrogates whether crime increases in response to unauthorized migrants and whether county officials are reasonable to invest in policing and the judiciary. Our crime information comes from the Jacob Kaplan's Concatenated Files, retrieved from the National Archive of Criminal Justice Data. This unofficial data-set condenses the information of yearly "Offenses Known and Clearances by Arrest (Return A)" by crime reported by the Uniform Crime Reporting Program Data. We use total crime, all crime included in the violent crime index, and all crime included in the property crime index. Since these crime data are noisy, we aggregate counts for 2010–11, 2014–15, and 2018–19. We construct our measures by dividing the counts by county population and then taking the natural logarithm.

Another last explanation for the shifting of votes in favor of the law-and-order party or police and judiciary spending is the demand for deportation of the unauthorized migrants. To examine this account we use the intensive margin of local participation in a federal deportation program called Secure Communities. We describe this program and some of its features in Appendix A. Secure Communities was subject to manipulation at the local level. Therefore, analyzing it allows us to distinguish among explanations of the shift to the political right. While investment in policing and the judiciary in response to the arrival of unauthorized migrants may be about fear (out-group bias), it could also be driven by populist backlash (Barone et al., 2016). If the shift is driven by a populist backlash, we would expect larger efforts to deport the unauthorized migrant population and more extensive use of the Secure Communities program.

We compile aggregated statistics from Secure Communities from October 2008 to September 30, 2013 (ICE 2013). We focus on four outcomes from the statistics. We use fingerprint submissions to capture local inquiries to ICE. Fingerprint matches are the subset of inquiries by local authorities for which ICE determines the individual is deportable. Removals are the subset of matches for which deportation actually occurs. Finally, we calculate the match success rate, which is the ratio of matches to submissions. We find suggestive evidence that deportation becomes more targeted with the arrival of new unauthorized migrants.

While detailed, the data source has a few shortcomings. Because of the timing of available data we can only estimate a cross section. Furthermore, while there is evidence that Secure Communities disproportionately targeted Hispanics, these data do not reflect Mexicans, but migrants of all nationalities who may be deportable. Additional limits and features of this data are discussed in the Appendix F.

Columns 1 through 4 in Table 23 display the results of the analysis on Secure Communities. The baseline OLS estimates in Panel A indicate that the arrival of more unauthorized migrants in a county is associated with an increase of fingerprints submissions, an increase in matches (with persons in ICE's database) and with subsequent removals (deportations). The second stage and reduced form estimates are generally larger in magnitude (Panel B and C). The second stage estimates suggest that in response to a mean inflow of migrants, police departments increase the number of fingerprint submissions per foreign born population by 33% (Panel B, Column 1, std coeff: 0.27). Furthermore, counties increased the number of matches from ICE by 62% (Panel B, Column 2, std coeff: 0.38), and subsequent removals (deportations) increased by 62%, as well (Panel B, Column 3, std coeff: 0.33).

Table 23: Effects of arrival of unauthorized Mexican migrants on crime (2010-2019) and on immigration enforcement (2008-2013)

	Count by foreign population (log)			Rate	Crime (log pc)		
	(1) Submissions	(2) Matches	(3) Removals	(4) Success	(5) All	(6) Violent	(7) Property
A. OLS							
Newcomers, pct. pop.	0.47***	0.98***	0.99***	0.52***	-0.01	-0.02	0.01
	(0.10)	(0.11)	(0.13)	(0.05)	(0.02)	(0.03)	(0.03)
B. 2SLS Loo							
Newcomers, pct. pop.	0.71***	1.31***	1.26***	0.62***	-0.02	-0.01	-0.01
	(0.11)	(0.13)	(0.16)	(0.05)	(0.02)	(0.03)	(0.03)
Std. Coefficient	0.26	0.38	0.33	0.37	-0.01	-0.01	-0.01
$\hat{\beta} * \bar{x}$	0.33	0.61	0.61	0.29	-0.01	-0.00	-0.00
C. 2SLS push factors							
Newcomers, pct. pop.	0.95***	1.64***	1.59***	0.71***	-0.04*	-0.01	-0.04
	(0.10)	(0.13)	(0.15)	(0.07)	(0.02)	(0.03)	(0.03)
Std. Coefficient	0.35	0.47	0.42	0.42	-0.03	-0.01	-0.02
$\hat{\beta} * \bar{x}$	0.44	0.76	0.77	0.33	-0.02	-0.00	-0.02
Observations	7964	7584	6068	7587	7847	7789	7830
Dep. Var., Mean	7.62	4.17	2.23	1.13	-3.49	-5.87	-3.89
Dep. Var., Sd	1.61	2.07	2.30	1.00	0.94	1.02	0.92
Ind. Var., Mean	0.46	0.47	0.48	0.47	0.46	0.46	0.46
Ind. Var., Sd	0.60	0.60	0.61	0.60	0.60	0.60	0.60
Inst., Mean	0.45	0.45	0.47	0.45	0.44	0.44	0.44
Inst., Sd	0.60	0.60	0.61	0.60	0.60	0.60	0.60

Dependent variables in columns 1-3 are submissions, matches and removals from the Secure Communities Program. These variables are calculated proportional to the time Secure Communities was in place in the county between 2008 and 2013 and proportional to the foreign population in the county in 2010. Dependent variable in column 4 is success rate –matched/submissions. Dependent variables in columns 5-7 are two year averages of the log of per capita total crime, violent crime index and property crime index. Sources: Jacob Kaplan's Concatenated Files: Uniform Crime Reporting Program Data: Offenses Known and Clearances by Arrest (Return A), 1960-2020; ICE: Secure Communities Monthly Statistics through September 30, 2013. Newcomers are the number of new consular IDs per county per 4-year period as a proportion of predicted population as described in Section 3. Source: SRE. Instrument is as described in Section 3. Standard errors are robust for the first four columns and clustered at the CBSA level for the last three. The first four estimations control for state fixed effects and the last three control for county and state-period fixed effects. Estimations weighted by predicted population. Stars indicate *p<0.1,**p<0.05,***p<0.01

These findings suggest that as more unauthorized migrants arrive in a county, police and sheriff's departments use Secure Communities more often and with greater accuracy. Indeed the success rate improves dramatically. Authorities both use the program more and use it better.

Since these estimates are based on a cross-section, we are hesitant to draw firm conclusions from the analysis. Nevertheless, the evidence does suggest a local approach to using Secure Communities that is actively anti-immigrant. We are exploring additional sources of data to investigate the relationships further.

Appendix U Robustness checks for mechanisms

Table 24: Robustness checks for mechanisms

	Emp (log)		Wages		County Economy (log)		Poor people (log)		Log of rate	Pop (Log)		Values
	(1) Const	(2) Hosp and leis	(3) Const	(4) Ag	(5) GDP pc	(6) Med HH inc	(7) Poor	(8) SNAP	(9) Out-mig	(10) Tot	(11) White	(12) Univ
A. Reduced form, baseline												
Instrument	-0.06***	-0.02**	-26.18*	-49.93**	-0.03	-0.03*	0.08***	0.03*	1.84**	-0.04***	-0.03***	-0.16***
	(0.02)	(0.01)	(14.80)	(22.42)	(0.02)	(0.02)	(0.01)	(0.02)	(0.72)	(0.01)	(0.01)	(0.05)
B. Lagged outcome (LO)												
Instrument	0.03	-0.01	-1.74	45.35**	-0.00	0.02**	-0.09***	-0.14***		0.01	0.03	
	(0.02)	(0.02)	(9.10)	(20.05)	(0.02)	(0.01)	(0.03)	(0.05)		(0.02)	(0.02)	
C. Mex non-citizen, sh												
Instrument	-0.14***	0.01	-56.51***	-34.68	-0.04*	-0.04**	0.06***	0.05*	2.22*	-0.07***	-0.04***	-0.21***
	(0.03)	(0.01)	(15.17)	(25.17)	(0.02)	(0.02)	(0.02)	(0.03)	(1.14)	(0.01)	(0.01)	(0.07)
D. Hispanics, sh												
Instrument	-0.08***	-0.01	-39.46***	-41.61*	-0.03	-0.02*	0.07***	0.04**	1.93**	-0.05***	-0.03***	-0.18***
	(0.02)	(0.01)	(13.40)	(21.78)	(0.02)	(0.01)	(0.02)	(0.02)	(0.77)	(0.01)	(0.01)	(0.05)
E. Adult HS completion												
Instrument	-0.07***	-0.02*	-34.09**	-44.29**	-0.03*	-0.03*	0.07***	0.03*	1.59**	-0.05***	-0.04***	-0.19***
	(0.02)	(0.01)	(14.08)	(22.12)	(0.02)	(0.02)	(0.01)	(0.02)	(0.70)	(0.01)	(0.01)	(0.05)
F. China shock												
Instrument	-0.06***	-0.02	-21.65	-54.66**	-0.02	-0.02	0.07***	0.05**	2.26***	-0.04***	-0.03***	-0.12**
	(0.02)	(0.01)	(15.00)	(23.29)	(0.02)	(0.02)	(0.01)	(0.02)	(0.76)	(0.01)	(0.01)	(0.05)
G. Simulated instrument												
Instrument	-0.17***	0.02	-24.52	-27.03	0.00	-0.01	0.05*	0.12***	4.32**	-0.07***	-0.05***	-0.28***
	(0.04)	(0.03)	(25.61)	(36.31)	(0.03)	(0.02)	(0.03)	(0.03)	(1.72)	(0.01)	(0.02)	(0.10)
H. Spatial lag												
Instrument	-0.06**	0.00	-16.45	-44.48*	-0.02	-0.02*	0.07***	0.04*	1.33*	-0.03*	-0.02	-0.14**
	(0.03)	(0.01)	(16.00)	(24.74)	(0.02)	(0.01)	(0.02)	(0.02)	(0.80)	(0.02)	(0.01)	(0.06)
I. Stock Mex foreign												
Instrument	-0.06***	-0.02**	-24.67*	-50.79**	-0.03	-0.02	0.07***	0.03	1.65**	-0.04***	-0.03***	-0.13**
	(0.02)	(0.01)	(14.78)	(23.12)	(0.02)	(0.02)	(0.01)	(0.02)	(0.72)	(0.01)	(0.01)	(0.05)
J. Stock Hispanics												
Instrument	-0.06***	-0.04**	-31.87**	-65.38***	-0.05**	-0.04***	0.09***	0.06**	1.00	-0.04***	-0.03**	-0.17***
	(0.02)	(0.02)	(14.58)	(25.04)	(0.02)	(0.02)	(0.02)	(0.02)	(0.76)	(0.01)	(0.01)	(0.05)
K. No-outliers												
Instrument	-0.07**	-0.04***	-30.75*	-61.81**	-0.04	-0.04**	0.09***	0.04	2.65***	-0.06***	-0.04***	-0.19***
	(0.03)	(0.01)	(17.19)	(30.17)	(0.03)	(0.02)	(0.02)	(0.03)	(0.98)	(0.01)	(0.01)	(0.07)
L. No pop weights												
Instrument	-0.09***	-0.01	-37.68***	-9.83	-0.02	-0.04***	0.05***	0.04*	0.31	-0.08***	-0.07***	-0.25***
	(0.03)	(0.01)	(11.12)	(13.70)	(0.02)	(0.01)	(0.01)	(0.02)	(1.93)	(0.01)	(0.01)	(0.09)
*M. County-group * period FE*												
Instrument	-0.02	-0.01	-8.87	38.85*	-0.05**	-0.03***	0.06***	0.05	-0.02	-0.06***	-0.06***	-0.11
	(0.04)	(0.02)	(17.33)	(20.53)	(0.02)	(0.01)	(0.02)	(0.03)	(3.04)	(0.01)	(0.01)	(0.14)

Dependent variables in columns 1–2 are the the log of average annual employment divided by working age population. Dependent variables in columns 3–4 are the annual average weekly wages in 2010 USD. Dependent variables in columns 5–6 are the log of GDP per capita and median household income. Dependent variables in columns 7–8 are the log of poor people and of SNAP recipients. The dependent variable in Column 9 is out-migration, calculated as the number of out-migrants (in thousands) divided by county population. Dependent variables in columns 10–11 are the log of total adults population and white adult population. Column 12 is the average county value, as reported by Enke (2020). All estimations are reduced form. Sources: Dave Leip's US Election Data; Annual Survey of State and Local Government Finances; US Census; ACS-5; USDA's Economic Research Service; Peter K. Schott's Data; County Business Patterns; Acemoglu et al. (2016); Enke (2020), and QCEW. Panel 1 is reduced form estimation. Panel 2 estimates effect on lagged dependent variables. Panel 3 controls for pre-2007 features interacted with period dummies. Panel 4 includes simulated instrument following Borusyak and Hull (2020). Panel 5 controls for the spatial lag of the instrument (values of neighboring counties). Panel 6 controls for the stock of Mexican non-citizens and Hispanics at the beginning of period. Panel 7 excludes outliers, does not use predicted population weights and instead of state-period fixed effects uses politically similar counties-period fixed effects. Standard errors clustered at CBSA level. Estimations control for county and state-period fixed effects, except for row M. Estimations weighted by predicted population, except row L. Stars indicate *p<0.1,**p<0.05,***p<0.01

Appendix V Robustness checks for mechanisms with push factor instrument

Table 25: Robustness checks for mechanisms, push factors instrument

	Emp (log)		Wages		County Economy (log)		Poor people (log)		Log of rate	Pop (Log)		Values
	(1) Const	(2) Hosp and leis	(3) Const	(4) Ag	(5) GDP pc	(6) Med HH inc	(7) Poor	(8) SNAP	(9) Out-mig	(10) Tot	(11) White	(12) Univ
A. Reduced form, baseline												
Instrument	-0.06*	-0.04***	-35.02***	-55.29**	-0.05***	-0.04**	0.09***	0.01	1.79**	-0.04***	-0.03**	-0.22***
	(0.03)	(0.01)	(12.80)	(25.55)	(0.02)	(0.02)	(0.02)	(0.03)	(0.81)	(0.01)	(0.01)	(0.06)
B. Lagged outcome (LO)												
Instrument	0.05**	-0.03	-4.81	45.97*	-0.01	0.03**	-0.11***	-0.14**		0.03	0.05**	
	(0.02)	(0.03)	(9.52)	(27.23)	(0.02)	(0.01)	(0.03)	(0.06)		(0.02)	(0.02)	
C. Mex non-citizen, sh												
Instrument	-0.14***	-0.01	-76.89***	-36.57	-0.07***	-0.06***	0.08***	0.01	1.98*	-0.07***	-0.04***	-0.31***
	(0.03)	(0.01)	(16.97)	(29.54)	(0.03)	(0.02)	(0.02)	(0.03)	(1.13)	(0.01)	(0.01)	(0.08)
D. Hispanics, sh												
Instrument	-0.08***	-0.02	-52.48***	-44.61*	-0.05**	-0.04**	0.08***	0.02	1.87**	-0.05***	-0.03**	-0.25***
	(0.03)	(0.01)	(11.75)	(24.36)	(0.02)	(0.02)	(0.02)	(0.02)	(0.84)	(0.01)	(0.01)	(0.06)
E. Adult HS completion												
Instrument	-0.07**	-0.03**	-44.97***	-47.56*	-0.06***	-0.04**	0.08***	0.01	1.47*	-0.05***	-0.04***	-0.27***
	(0.03)	(0.01)	(12.20)	(24.88)	(0.02)	(0.02)	(0.02)	(0.02)	(0.78)	(0.01)	(0.01)	(0.06)
F. China shock												
Instrument	-0.05*	-0.03**	-29.85**	-64.81**	-0.04**	-0.03*	0.09***	0.03	2.24***	-0.03**	-0.03**	-0.17***
	(0.03)	(0.01)	(13.10)	(27.06)	(0.02)	(0.02)	(0.02)	(0.03)	(0.87)	(0.01)	(0.01)	(0.06)
G. Simulated instrument												
Instrument	-0.20***	0.00	-54.55**	-19.64	-0.06	-0.03	0.06*	0.06	5.05*	-0.07***	-0.05*	-0.60***
	(0.04)	(0.03)	(24.66)	(38.98)	(0.04)	(0.02)	(0.04)	(0.04)	(2.68)	(0.02)	(0.02)	(0.13)
H. Spatial lag												
Instrument	-0.05*	-0.00	-23.63	-49.44	-0.04*	-0.04**	0.09***	0.01	1.01	-0.02	-0.01	-0.21***
	(0.03)	(0.01)	(15.79)	(30.75)	(0.02)	(0.02)	(0.02)	(0.02)	(0.99)	(0.02)	(0.02)	(0.07)
I. Stock Mex foreign												
Instrument	-0.06**	-0.04***	-33.42**	-58.29**	-0.04**	-0.03**	0.08***	0.00	1.51*	-0.04***	-0.03**	-0.18***
	(0.03)	(0.01)	(13.06)	(27.43)	(0.02)	(0.02)	(0.02)	(0.03)	(0.80)	(0.01)	(0.01)	(0.06)
J. Stock Hispanics												
Instrument	-0.05*	-0.05***	-39.08***	-64.57**	-0.07***	-0.05***	0.10***	0.03	1.09	-0.04***	-0.03**	-0.23***
	(0.03)	(0.02)	(13.15)	(27.34)	(0.02)	(0.02)	(0.02)	(0.03)	(0.90)	(0.01)	(0.01)	(0.06)
K. No-outliers												
Instrument	-0.06	-0.06***	-39.55**	-69.63**	-0.06**	-0.05***	0.11***	0.02	2.31**	-0.06***	-0.04***	-0.24***
	(0.04)	(0.02)	(16.11)	(34.03)	(0.03)	(0.02)	(0.02)	(0.03)	(0.98)	(0.02)	(0.01)	(0.08)
L. No pop weights												
Instrument	-0.11***	-0.02	-42.33***	-17.92	-0.02	-0.05***	0.07***	0.03	0.61	-0.09***	-0.08***	-0.31***
	(0.04)	(0.02)	(14.67)	(14.90)	(0.03)	(0.01)	(0.01)	(0.02)	(3.18)	(0.01)	(0.01)	(0.11)
*M. County-group * period FE*												
Instrument	-0.03	-0.01	-15.41	29.26	-0.08**	-0.04***	0.06***	0.04	0.75	-0.08***	-0.07***	-0.12
	(0.05)	(0.03)	(20.31)	(22.72)	(0.03)	(0.01)	(0.02)	(0.03)	(4.30)	(0.01)	(0.01)	(0.18)

Dependent variables in columns 1–2 are the the log of average annual employment divided by working age population. Dependent variables in columns 3–4 are the annual average weekly wages in 2010 USD. Dependent variables in columns 5–6 are the log of GDP per capita and median household income. Dependent variables in columns 7–8 are the log of poor people and of SNAP recipients. The dependent variable in Column 9 is out-migration, calculated as the number of out-migrants (in thousands) divided by county population. Dependent variables in columns 10–11 are the log of total adults population and white adult population. Column 12 is the average county value, as reported by Enke (2020). All estimations are reduced form. Sources: Dave Leip's US Election Data; Annual Survey of State and Local Government Finances; US Census; ACS-5; USDA's Economic Research Service; Peter K. Schott's Data; County Business Patterns; Acemoglu et al. (2016); Enke (2020), and QCEW. Panel 1 is reduced form estimation. Panel 2 estimates effect on lagged dependent variables. Panel 3 controls for pre-2007 features interacted with period dummies. Panel 4 includes simulated instrument following Borusyak and Hull (2020). Panel 5 controls for the spatial lag of the instrument (values of neighboring counties). Panel 6 controls for the stock of Mexican non-citizens and Hispanics at the beginning of period. Panel 7 excludes outliers, does not use predicted population weights and instead of state-period fixed effects uses politically similar counties-period fixed effects. Standard errors clustered at CBSA level. Estimations control for county and state-period fixed effects, except for row M. Estimations weighted by predicted population, except row L. Stars indicate *p<0.1,**p<0.05,***p<0.01

Appendix W Robustness checks of null results

Table 26: Robustness checks for null results

	Share of Dir Exp	Midterms	Employment (log)		Weekly Wages (2010 USD)			Rate	Crime (log pc)		
	(1) Edu	(2) Turnout	(3) Total	(4) Agric	(5) Total	(6) Manufac	(7) Hosp and leis	(8) Unemployment	(9) All	(10) Violent	(11) Property
A. Reduced form, baseline											
Instrument	0.27	-0.81	-0.00	-2.06	18.57	-9.35	-49.93**	0.20	-0.03	-0.01	-0.01
	(0.71)	(0.96)	(0.01)	(21.62)	(38.74)	(7.42)	(22.42)	(0.21)	(0.03)	(0.03)	(0.03)
B. Lagged outcome (LO)											
Instrument	-1.38**	1.00	0.02	-20.81**	-8.01	-17.55***	45.35**	-0.34	0.07*	0.10	0.07
	(0.64)	(1.14)	(0.02)	(10.14)	(15.00)	(5.96)	(20.05)	(0.27)	(0.04)	(0.06)	(0.04)
C. Mex non-citizen, sh											
Instrument	-0.40	-1.61	-0.01	-46.13**	-4.90	-21.43***	-34.68	0.07	-0.04	-0.01	-0.04
	(1.08)	(1.45)	(0.01)	(19.20)	(40.56)	(6.60)	(25.17)	(0.23)	(0.03)	(0.04)	(0.04)
D. Hispanics, sh											
Instrument	-0.03	-1.23	0.00	-12.28	14.59	-7.84	-41.61*	0.05	-0.01	-0.02	0.02
	(0.78)	(1.20)	(0.01)	(18.76)	(35.51)	(7.10)	(21.78)	(0.19)	(0.03)	(0.03)	(0.03)
E. Adult HS completion											
Instrument	0.25	-1.15	-0.00	-7.69	16.94	-9.79	-44.29**	-0.03	-0.03	-0.01	-0.01
	(0.72)	(0.99)	(0.01)	(20.35)	(37.43)	(7.06)	(22.12)	(0.17)	(0.03)	(0.03)	(0.03)
F. China shock											
Instrument	-0.21	-0.33	-0.00	5.71	26.77	-4.84	-54.66**	0.35	-0.01	-0.03	0.02
	(0.73)	(1.02)	(0.01)	(22.20)	(41.43)	(7.36)	(23.29)	(0.22)	(0.03)	(0.04)	(0.03)
G. Simulated instrument											
Instrument	-1.76	-1.99	-0.02	19.43	143.15	7.68	-27.03	1.02**	0.13***	-0.01	0.18***
	(1.54)	(2.45)	(0.02)	(45.08)	(95.00)	(11.50)	(36.31)	(0.42)	(0.05)	(0.07)	(0.06)
H. Spatial lag											
Instrument	0.17	-1.32	0.00	-5.15	26.05	-6.57	-44.48*	0.36	-0.02	-0.01	-0.01
	(1.02)	(1.53)	(0.01)	(17.96)	(39.97)	(7.50)	(24.74)	(0.23)	(0.03)	(0.04)	(0.03)
I. Stock Mex foreign											
Instrument	0.19	-0.53	-0.00	-0.51	19.00	-9.17	-50.79**	0.22	-0.02	0.00	-0.00
	(0.69)	(0.94)	(0.01)	(20.85)	(38.97)	(7.43)	(23.12)	(0.21)	(0.03)	(0.03)	(0.03)
J. Stock Hispanics											
Instrument	0.84	-1.75**	-0.01	-15.55	-5.34	-16.54**	-65.38***	0.17	-0.04	-0.03	-0.02
	(0.78)	(0.85)	(0.01)	(20.87)	(37.37)	(8.10)	(25.04)	(0.22)	(0.03)	(0.04)	(0.03)
K. No-outliers											
Instrument	0.71	0.89	-0.00	-9.73	1.75	-16.06**	-61.81**	0.54**	-0.02	-0.00	-0.01
	(0.87)	(0.76)	(0.01)	(26.92)	(50.07)	(7.23)	(30.17)	(0.24)	(0.04)	(0.04)	(0.05)
L. No pop weights											
Instrument	-1.29	-1.57***	0.01	-15.14**	21.93	-3.95	-9.83	-0.26*	0.02	0.04	0.02
	(0.96)	(0.57)	(0.01)	(6.29)	(17.31)	(3.13)	(13.70)	(0.16)	(0.05)	(0.06)	(0.05)
*M. County-group * period FE*											
Instrument	-1.08	-1.42*	-0.00	-4.17	39.94	2.96	38.85*	-0.16	-0.06	-0.03	-0.04
	(1.30)	(0.81)	(0.02)	(13.04)	(30.56)	(6.01)	(20.53)	(0.17)	(0.07)	(0.09)	(0.08)

Dependent variables in column 1 is education expenditure as total direct expenditures. Dependent variable in column 2 is turnout in House midterm elections, as share of registered voters. Dependent variables in columns 3–4 are the log of average annual employment divided by working age population. Dependent variables in columns 5–8 are the annual average weekly wages in 2010 USD. Dependent variables in columns 9–11 are two year averages of the log of per capita total crime, violent crime index and property crime index. Sources: Annual Survey of State and Local Government Finances; Dave Leip's US Election Data; Quarterly Census of Employment and Wages. Jacob Kaplan's Concatenated Files: Uniform Crime Reporting Program Data: Offenses Known and Clearances by Arrest (Return A), 1960-2020; US Census; ACS-5; USDA's Economic Research Service; Peter K. Schott's Data; County Business Patterns; Acemoglu et al. (2016). Panel 1 is reduced form estimation. Panel 2 estimates effect on lagged dependent variables. Panel 3 controls for pre-2007 features interacted with period dummies. Panel 4 includes simulated instrument following Borusyak and Hull (2020). Panel 5 controls for the spatial lag of the instrument (values of neighboring counties). Panel 6 controls for the stock of Mexican non-citizens and Hispanics at the beginning of period. Panel 7 excludes outliers, does not use predicted population weights and instead of state-period fixed effects uses politically similar counties-period fixed effects. Standard errors clustered at CBSA level. Estimations control for county and state-period fixed effects, except for row M. Estimations weighted by predicted population, except row L. Stars indicate *p<0.1,**p<0.05,***p<0.01

Table 27: Robustness checks for null results

	Share of Dir Exp	Midterms	Employment (log)		Weekly Wages (2010 USD)			Rate	Crime (log pc)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Edu	Turnout	Total	Agric	Total	Manufac	Hosp and leis	Unemployment	All	Violent	Property
A. Reduced form, baseline											
Instrument	1.15*	-0.71	-0.00	-18.19	-14.83	-13.23*	-55.29**	0.08	-0.06*	-0.01	-0.05
	(0.65)	(0.77)	(0.01)	(16.74)	(27.94)	(8.02)	(25.55)	(0.25)	(0.03)	(0.04)	(0.04)
B. Lagged outcome (LO)											
Instrument	-1.81***	3.01*	0.02	-24.88**	-20.21	-26.75***	45.97*	-0.71**	0.12**	0.20**	0.11**
	(0.60)	(1.70)	(0.02)	(9.88)	(19.90)	(5.55)	(27.23)	(0.32)	(0.05)	(0.09)	(0.05)
C. Mex non-citizen, sh											
Instrument	0.95	-1.74	-0.02	-84.57***	-73.82	-31.73***	-36.57	-0.21	-0.11***	-0.00	-0.12***
	(0.91)	(1.46)	(0.01)	(24.80)	(50.70)	(8.55)	(29.54)	(0.29)	(0.04)	(0.05)	(0.04)
D. Hispanics, sh											
Instrument	0.89	-1.23	-0.00	-33.13**	-24.40	-11.98	-44.61*	-0.14	-0.04	-0.03	-0.02
	(0.71)	(1.11)	(0.01)	(14.75)	(27.09)	(8.00)	(24.36)	(0.25)	(0.03)	(0.04)	(0.04)
E. Adult HS completion											
Instrument	1.14*	-1.14	-0.00	-25.40*	-17.00	-13.75*	-47.56*	-0.23	-0.06**	-0.02	-0.05
	(0.66)	(0.85)	(0.01)	(15.31)	(27.35)	(7.54)	(24.88)	(0.25)	(0.03)	(0.04)	(0.03)
F. China shock											
Instrument	0.59	-0.23	-0.01	-8.91	-7.18	-7.61	-64.81**	0.27	-0.04	-0.04	-0.01
	(0.68)	(0.85)	(0.01)	(17.35)	(31.40)	(7.88)	(27.06)	(0.26)	(0.03)	(0.04)	(0.04)
G. Simulated instrument											
Instrument	1.05	-1.97	-0.03	-43.12	62.25	5.39	-19.64	0.88	0.09	-0.01	0.15
	(1.72)	(2.30)	(0.02)	(37.18)	(57.48)	(14.71)	(38.98)	(0.56)	(0.08)	(0.08)	(0.09)
H. Spatial lag											
Instrument	1.36	-1.40	0.00	-28.83**	-16.33	-10.49	-49.44	0.26	-0.06*	-0.01	-0.05
	(1.16)	(1.66)	(0.01)	(13.24)	(27.09)	(9.28)	(30.75)	(0.27)	(0.03)	(0.05)	(0.04)
I. Stock Mex foreign											
Instrument	0.93	-0.24	-0.00	-16.79	-16.33	-13.27*	-58.29**	0.09	-0.04	0.01	-0.04
	(0.65)	(0.72)	(0.01)	(14.87)	(29.31)	(8.01)	(27.43)	(0.25)	(0.03)	(0.04)	(0.04)
J. Stock Hispanics											
Instrument	1.64**	-1.39**	-0.01	-29.34	-35.14	-18.76**	-64.57**	0.04	-0.07**	-0.03	-0.06
	(0.77)	(0.60)	(0.02)	(20.26)	(30.22)	(9.19)	(27.34)	(0.27)	(0.03)	(0.04)	(0.04)
K. No-outliers											
Instrument	1.98**	1.58*	-0.00	-32.16	-47.19	-21.73***	-69.63**	0.37	-0.07	-0.01	-0.06
	(0.87)	(0.88)	(0.02)	(21.98)	(38.03)	(6.84)	(34.03)	(0.27)	(0.05)	(0.06)	(0.05)
L. No pop weights											
Instrument	-1.08	-1.83***	0.02	-30.10***	3.34	-5.09	-17.92	-0.41**	0.04	0.06	0.04
	(1.13)	(0.71)	(0.01)	(7.91)	(19.40)	(4.02)	(14.90)	(0.19)	(0.05)	(0.07)	(0.05)
*M. County-group * period FE*											
Instrument	-0.80	-1.41	0.00	-20.30	37.26	4.64	29.26	-0.36*	-0.05	0.01	-0.03
	(1.53)	(0.99)	(0.02)	(13.24)	(29.78)	(8.12)	(22.72)	(0.21)	(0.09)	(0.11)	(0.09)

Dependent variables in column 1 is education expenditure as total direct expenditures. Dependent variable in column 2 is turnout in House midterm elections, as share of registered voters. Dependent variables in columns 3–4 are the log of average annual employment divided by working age population. Dependent variables in columns 5–8 are the annual average weekly wages in 2010 USD. Dependent variables in columns 9–11 are two year averages of the log of per capita total crime, violent crime index and property crime index. Sources: Annual Survey of State and Local Government Finances; Dave Leip's US Election Data; Quarterly Census of Employment and Wages. Jacob Kaplan's Concatenated Files: Uniform Crime Reporting Program Data: Offenses Known and Clearances by Arrest (Return A), 1960-2020; US Census; ACS-5; USDA's Economic Research Service; Peter K. Schott's Data; County Business Patterns; Acemoglu et al. (2016). Panel 1 is reduced form estimation. Panel 2 estimates effect on lagged dependent variables. Panel 3 controls for pre-2007 features interacted with period dummies. Panel 4 includes simulated instrument following Borusyak and Hull (2020). Panel 5 controls for the spatial lag of the instrument (values of neighboring counties). Panel 6 controls for the stock of Mexican non-citizens and Hispanics at the beginning of period. Panel 7 excludes outliers, does not use predicted population weights and instead of state-period fixed effects uses politically similar counties-period fixed effects. Standard errors clustered at CBSA level. Estimations control for county and state-period fixed effects, except for row M. Estimations weighted by predicted population, except row L. Stars indicate *p<0.1,**p<0.05,***p<0.01

Appendix X Heterogeneous effects by tax progressivity/safety nets

Table 28: Main effects and mechanisms by ratio of income vs sales tax

	GOP vote	Log pc	Share of Expend		Emp (log)	Wages 2010 USD	County Economy (log)			Log		Values
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	House	Edu	Police	Judicial	Const	Const	Med hh inc	GDP	Poor	Out-mig	White pop	Univ
A. OLS												
Newcomers, pct. pop.	7.66***	-0.01	0.19	0.10	-0.05**	-40.46***	-0.03**	-0.02	0.05***	0.01	-0.03**	-0.13**
	(0.91)	(0.02)	(0.18)	(0.20)	(0.02)	(8.76)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.06)
Above-median	-2.60	-0.01	-0.23	0.00	0.07***	49.85***	0.04*	0.03	-0.02	0.04	0.02	0.10
ratio × Newcomers, pct. pop.	(1.78)	(0.03)	(0.24)	(0.21)	(0.03)	(18.80)	(0.02)	(0.03)	(0.02)	(0.02)	(0.01)	(0.09)
B. Reduced form Loo												
Newcomers, pct. pop.	13.05***	-0.04	0.45*	0.18	-0.08***	-71.93***	-0.04**	-0.04	0.09***	0.02	-0.04	-0.25***
	(1.41)	(0.04)	(0.25)	(0.27)	(0.03)	(11.43)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.08)
Above-median	-5.05**	-0.01	-0.29	0.11	0.10***	82.87***	0.05	0.05	-0.02	0.05*	0.02	0.22**
ratio× Newcomers, pct. pop.	(2.32)	(0.05)	(0.33)	(0.29)	(0.04)	(25.90)	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)	(0.11)
C. Reduced form push factor												
Newcomers, pct. pop.	14.18***	-0.03	0.56**	0.18	-0.11***	-82.24***	-0.04**	-0.04	0.10***	0.02	-0.04	-0.31***
	(1.81)	(0.03)	(0.26)	(0.26)	(0.03)	(14.92)	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)	(0.09)
Above-median	-6.74**	0.01	-0.53	0.01	0.16***	80.92***	0.03	0.02	-0.03	0.06*	0.03	0.20
ratio × Newcomers, pct. pop.	(2.75)	(0.04)	(0.37)	(0.28)	(0.05)	(23.43)	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)	(0.13)
Observations	7728	5158	5164	5100	7141	7745	7752	7617	7752	7752	7752	5508
Dep. Var., Mean	48.95	1.96	5.47	1.41	-3.60	980.17	10.88	3.90	10.87	-2.94	12.35	0.14
Dep. Var., Sd	18.84	0.34	1.85	0.85	0.44	241.71	0.26	0.43	1.61	0.28	1.53	0.49

Dependent variable in column 1 is the share of GOP vote in midterm House elections. Dependent variable in column 2 is the in log 2010 dollars if per child education expenditures. Dependent variables in columns 3 and 4 are shares of total direct expenditures in the police and the judiciary respectively. Dependent variable in columns 5 and 6 are the log of per working age employment rate in manufacturing and construction. Dependent variable in column 7 is the log of median household income. Dependent variable in column 8 is the log of unemployment rate. Dependent variable in column 9 is the log of poverty rate. Dependent variable in column 10 is the log of outmigration rate. Dependent variable in column 11 is the log of white adult population. Dependent variable in column 12 is the prevalence of universalist values following Enke (2020). Panels B and C are reduced form. We interact the corresponding instruments and fixed effects with an indicator of whether the county is above or below the relative contribution of income vs sales tax in 2007. Above equals more importance of income tax, which suggests a more progressive fiscal policy. Above suggests a more progressive fiscal policy. Sources: Enke, (2020); US Census Bureau: Population Division and Small Area; US Census Bureau: 2007-2011, 2011-2015, and 2015-2019 American Community Surveys 5; QCEW; Small Area Income and Poverty Estimates (SAIPE) Program; US Department of Commerce: Bureau of Economic Analysis. Regressions are reduced form. Newcomers are the number of new consular IDs per county per 4-year period as a proportion of predicted population as described in Section 3. Source: SRE. Instrument is as described in Section 3. Standard errors clustered at the CBSA level. All estimations control for county and state-period fixed effects. Estimations weighted by predicted population. Stars indicate *p<0.1,**p<0.05,***p<0.01

Table 29: Main effects and mechanisms by state tax inequality

	GOP vote	Log pc	Share of Expend		Emp (log)	Wages 2010 USD	County Economy (log)			Log		Values
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	House	Edu	Police	Judicial	Const	Const	Med hh inc	GDP	Poor	Out-mig	White pop	Univ
A. OLS												
Newcomers, pct. pop.	6.31***	-0.04*	0.39***	0.23**	-0.06***	-29.84***	-0.03***	-0.02	0.05***	0.02*	-0.03***	-0.15**
	(0.84)	(0.02)	(0.15)	(0.12)	(0.02)	(9.92)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.06)
Above-median ratio	0.39	0.02	-0.34	-0.19	0.06*	25.86	0.02	0.01	-0.02	0.01	0.02	0.12
× Newcomers, pct. pop.	(1.74)	(0.03)	(0.22)	(0.17)	(0.03)	(18.59)	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.08)
B. Reduced form Loo												
Newcomers, pct. pop.	8.98***	-0.06**	0.76***	0.45***	-0.09***	-49.73***	-0.05***	-0.04*	0.09***	0.03**	-0.04*	-0.24***
	(1.29)	(0.03)	(0.22)	(0.13)	(0.03)	(14.49)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.07)
Above-median ratio	1.81	-0.00	-0.52	-0.27	0.07	43.72	0.05	0.02	-0.03	0.02	0.02	0.16
× Newcomers, pct. pop.	(2.51)	(0.04)	(0.32)	(0.23)	(0.04)	(28.23)	(0.03)	(0.04)	(0.03)	(0.03)	(0.02)	(0.10)
C. Reduced form push factor												
Newcomers, pct. pop.	9.77***	-0.05*	0.83***	0.44***	-0.11***	-53.42***	-0.05***	-0.05*	0.11***	0.03	-0.04*	-0.31***
	(1.45)	(0.03)	(0.25)	(0.14)	(0.03)	(16.83)	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.09)
Above-median ratio	1.28	0.02	-0.69*	-0.35	0.11**	31.93	0.03	0.01	-0.04	0.04	0.02	0.18
× Newcomers, pct. pop.	(3.04)	(0.04)	(0.38)	(0.24)	(0.05)	(25.95)	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)	(0.12)
Observations	7995	5328	5334	5266	7388	8008	8019	7884	8019	8017	8019	5712
Dep. Var., Mean	48.16	1.96	5.45	1.41	-3.62	983.38	10.88	3.89	10.89	-2.94	12.36	0.15
Dep. Var., Sd	19.44	0.34	1.85	0.85	0.44	241.74	0.26	0.44	1.63	0.28	1.52	0.50

Dependent variable in column 1 is the share of GOP vote in midterm House elections. Dependent variable in column 2 is the in log 2010 dollars if per child education expenditures. Dependent variables in columns 3 and 4 are shares of total direct expenditures in the police and the judiciary respectively. Dependent variable in columns 5 and 6 are the log of per working age employment rate in manufacturing and construction. Dependent variable in column 7 is the log of median household income. Dependent variable in column 8 is the log of unemployment rate. Dependent variable in column 9 is the log of poverty rate. Dependent variable in column 10 is the log of outmigration rate. Dependent variable in column 11 is the log of white adult population. Dependent variable in column 12 is the prevalence of universalist values following Enke (2020). Panels B and C are reduced form. We interact the corresponding instruments and fixed effects with an indicator of whether the county is in a state with above or below the median of tax equality according to the Institute for Taxation and Economic Policy. Above suggests a more progressive fiscal policy. Sources: Enke, (2020); US Census Bureau: Population Division and Small Area; US Census Bureau: 2007-2011, 2011-2015, and 2015-2019 American Community Surveys 5; QCEW; Small Area Income and Poverty Estimates (SAIPE) Program; US Department of Commerce: Bureau of Economic Analysis. Regressions are reduced form. Newcomers are the number of new consular IDs per county per 4-year period as a proportion of predicted population as described in Section 3. Source: SRE. Instrument is as described in Section 3. Standard errors clustered at the CBSA level. All estimations control for county and state-period fixed effects. Estimations weighted by predicted population. Stars indicate *p<0.1,**p<0.05,***p<0.01

Table 30: Main effects and mechanisms by state Tanf-poverty ratio

	GOP vote	Log pc	Share of Expend		Emp (log)	Wages 2010 USD	County Economy (log)			Log		Values
	(1) House	(2) Edu	(3) Police	(4) Judicial	(5) Const	(6) Const	(7) Med hh inc	(8) GDP	(9) Poor	(10) Out-mig	(11) White pop	(12) Univ
A. OLS												
Newcomers, pct. pop.	6.66***	-0.03**	0.30**	0.10	-0.05***	-31.64***	-0.03***	-0.02	0.05***	0.02*	-0.03***	-0.14***
	(0.81)	(0.02)	(0.14)	(0.14)	(0.02)	(9.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.05)
Above-median	-0.42	0.01	-0.23	0.07	0.05	41.11*	0.05**	0.02	-0.02	0.03	0.03**	0.13*
ratio × Newcomers, pct. pop.	(2.07)	(0.03)	(0.24)	(0.16)	(0.03)	(21.94)	(0.02)	(0.03)	(0.02)	(0.02)	(0.01)	(0.08)
B. Reduced form Loo												
Newcomers, pct. pop.	9.56***	-0.06**	0.63***	0.27	-0.08***	-51.96***	-0.05***	-0.04**	0.09***	0.03**	-0.04**	-0.24***
	(1.37)	(0.02)	(0.22)	(0.18)	(0.02)	(13.42)	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)	(0.07)
Above-median	0.83	-0.00	-0.35	0.11	0.05	65.58**	0.07**	0.04	-0.03	0.04	0.03	0.22**
ratio × Newcomers, pct. pop.	(2.71)	(0.05)	(0.32)	(0.21)	(0.04)	(32.89)	(0.03)	(0.05)	(0.03)	(0.03)	(0.02)	(0.10)
C. Reduced form push factor												
Newcomers, pct. pop.	10.95***	-0.05*	0.68***	0.30	-0.10***	-59.23***	-0.06***	-0.05*	0.10***	0.03**	-0.04**	-0.30***
	(1.58)	(0.02)	(0.23)	(0.18)	(0.03)	(16.21)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.08)
Above-median	-1.43	0.02	-0.51	-0.08	0.11*	56.44**	0.05	0.00	-0.03	0.04	0.04	0.21*
ratio × Newcomers, pct. pop.	(3.23)	(0.04)	(0.39)	(0.22)	(0.06)	(27.39)	(0.03)	(0.04)	(0.03)	(0.03)	(0.02)	(0.12)
Observations	7995	5328	5334	5266	7388	8008	8019	7884	8019	8017	8019	5712
Dep. Var., Mean	48.16	1.96	5.45	1.41	-3.62	983.38	10.88	3.89	10.89	-2.94	12.36	0.15
Dep. Var., Sd	19.44	0.34	1.85	0.85	0.44	241.74	0.26	0.44	1.63	0.28	1.52	0.50

Dependent variable in column 1 is the share of GOP vote in midterm House elections. Dependent variable in column 2 is the in log 2010 dollars if per child education expenditures. Dependent variables in columns 3 and 4 are shares of total direct expenditures in the police and the judiciary respectively. Dependent variable in columns 5 and 6 are the log of per working age employment rate in manufacturing and construction. Dependent variable in column 7 is the log of median household income. Dependent variable in column 8 is the log of unemployment rate. Dependent variable in column 9 is the log of poverty rate. Dependent variable in column 10 is the log of outmigration rate. Dependent variable in column 11 is the log of white adult population. Dependent variable in column 12 is the prevalence of universalist values following Enke (2020). Panels B and C are reduced form. We interact the corresponding instruments and fixed effects with an indicator of whether the county is in state with above or below the median of Tanf-poverty ratio according to the Center for American Progress. Above suggests a more progressive fiscal policy. Sources: Enke, (2020); US Census Bureau: Population Division and Small Area; US Census Bureau: 2007-2011, 2011-2015, and 2015-2019 American Community Surveys 5; QCEW; Small Area Income and Poverty Estimates (SAIPE) Program; US Department of Commerce: Bureau of Economic Analysis. Regressions are reduced form. Newcomers are the number of new consular IDs per county per 4-year period as a proportion of predicted population as described in Section 3. Source: SRE. Instrument is as described in Section 3. Standard errors clustered at the CBSA level. All estimations control for county and state-period fixed effects. Estimations weighted by predicted population. Stars indicate *p<0.1,**p<0.05,***p<0.01

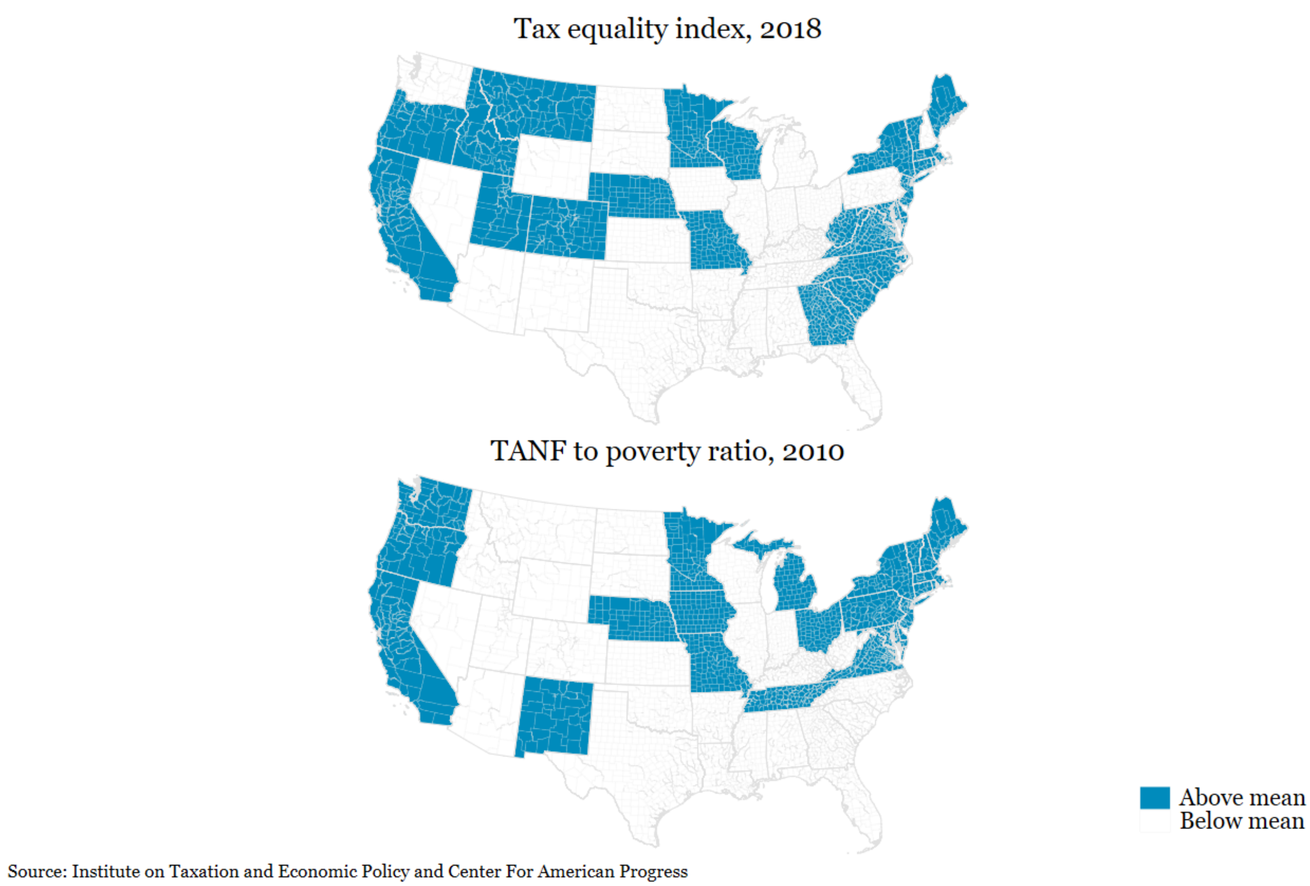

Figure 16: Map of categories of states

Appendix Y Heterogeneous effects

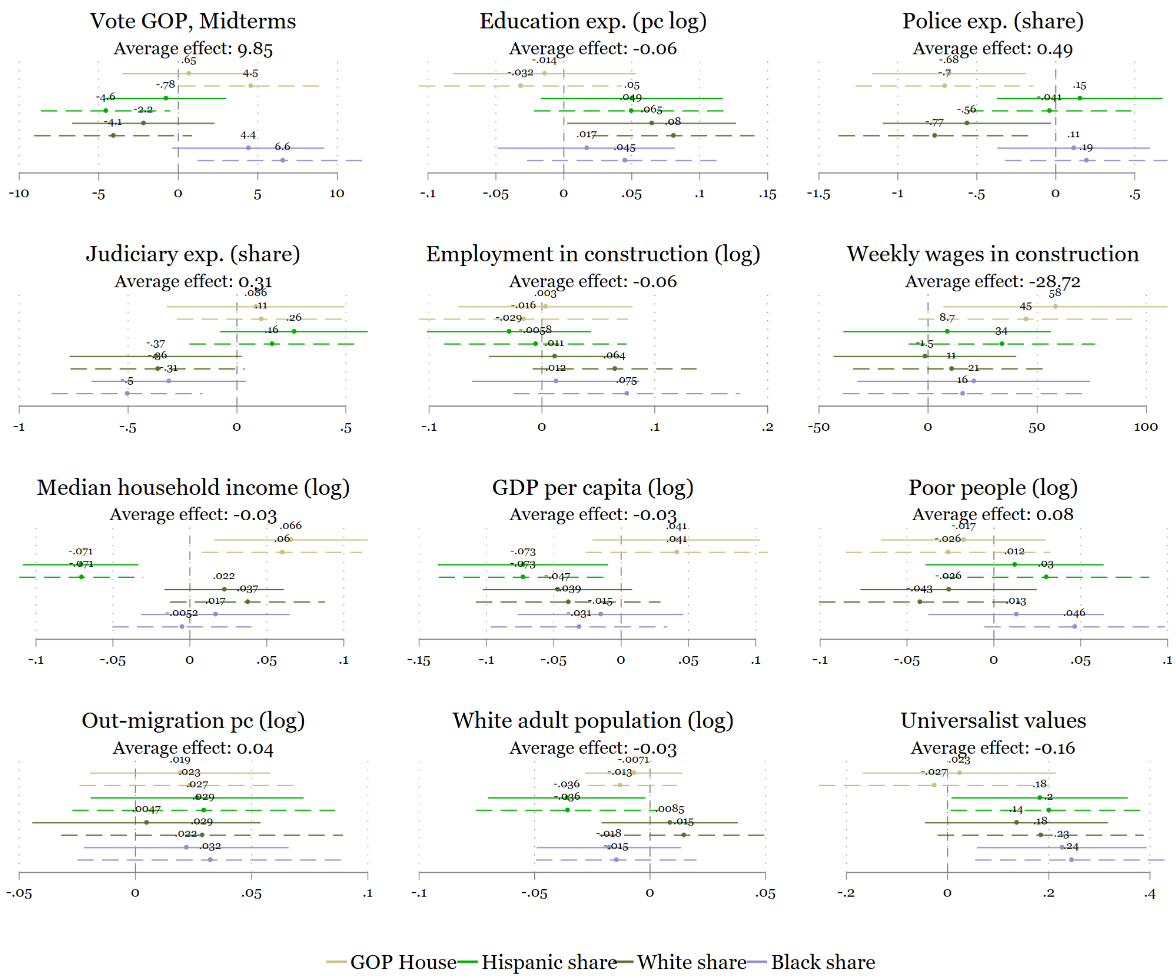

Figure 17: Heterogeneous effects by pre-period tax progressivity/strength of the safety net. Displayed are the 90% coefficient intervals of the interaction between the instrument(s) and dummy indicating above median values. Estimations are reduced form. Solid lines correspond to the LOO instrument and dashed lines to the push factors instrument.